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DIVIDING THE SPOILS: AN EMPIRICAL STUDY OF TWO INDUSTRIES

by

David B. Marzo
A Dissertation
Submitted to the
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of
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in Partial Fulfillment of
the Requirements for the Degree
of
Doctor of Philosophy
Economics

Committee:

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Summer Semester 2002
George Mason University
Fairfax, Virginia

Dividing The Spoils: An Empirical Study Of Two Industries

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy at George Mason University.

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Abstract

DIVIDING THE SPOILS: AN EMPIRICAL EXAMINATION OF TWO INDUSTRIES

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This dissertation examines the forces that influence the distribution of money in both the sports industry and the defense industry. The method to test for these influences is empirical. Both industries offer ample data sets that facilitate testing of the hypothesized influences.

Chapter Two addresses the impact of new professional sports venues on athletes' salaries. In particular, the case of baseball is considered. To this point, the research on new baseball stadiums has focused primarily on a stadium's impact on local communities. This research is novel in shifting the focus to the impact of a new stadium on the athlete. Using data from 1990 to 1999, it is shown that players, particularly pitchers, do in fact receive a higher salary when playing for a team in a new stadium, ceteris paribus. Furthermore, it is shown that this higher salary is driven by rent sharing between player and owner. This conclusion results from the evidence that shows that a new stadium has a slight negative impact on the marginal productivity of the player.

Chapter Three examines the extent to which the distribution of defense spending is dictated by political forces. This research extends previous work in which political

variables are shown to have some impact in the distribution of defense prime contracts by state. By extending the data series to include the 1990's, trends that were apparent from 1963 to 1989, appear to no longer hold. This result may be the function of an institutional shift resulting from the fall of the Soviet threat in the 1990s.

Yet, this research is criticized for using a dependent variable that is too highly aggregated. In light of this criticism, an alternative dependent variable is proposed in which the issue of aggregated data is no longer a concern. By considering the case of military construction earmarks for the 104th through 106th Congress, a distinct set of political influences manifest themselves. Members of the relevant military construction subcommittees appear to have a significant advantage in obtaining earmarks. Close district elections also indicate increased earmarks within that district while representative seniority appears to play little explanatory role.

Chapter 1: Introduction

The objective of this research is to examine the "division of spoils" in two industries, namely baseball and defense. These industries, while seemingly disparate, both offer unique opportunities to examine the flow of money within industry, thereby expanding the existing literature in innovative ways. In addition, both topics will be considered from a Public Choice orientation, in which this distribution of money is hypothesized to be a function of some politically related variable(s). For baseball, the question is how baseball players benefit from the construction of new athletic venues, and specifically whether team owners distribute revenue they receive from publicly-financed stadiums to the players by way of increased salary. For defense, the question is whether defense contracts flow at a higher rate to geographic regions that are represented on defense-related committees within Congress.

Chapter Two considers the case of baseball and the impact of new stadiums on player salaries. Over the past decade, almost half of the major league teams have moved into new stadiums, the vast majority built with public funds. In part, baseball's success in obtaining public funds derives from legal protection it has received from the government with respect to antitrust. Major League Baseball has enjoyed immunity from antitrust prosecution since 1922 when the Supreme Court declared in *Federal Club v. National League* that baseball was an affair of the state and thus not subject to the Sherman Antitrust Act. Part of the exemption involves the league's ability to control franchise expansion and re-location. The league has used this power to make credible threats to re-locate teams when the host city has balked at providing public funds to either construct or support the construction of a new stadium for the team.[1,2] For the purposes of this paper, the ability of baseball to extract public subsidies for stadium construction is considered a monopoly-generated rent. The "rent" is either a monetary return to the owner when the owner shares in the public stadium-generated revenue or it manifests itself as a reduced cost to the owner since the owner does not have to spend team funds for the stadium even though the stadium is a vital factor of production.[3]

Conventional economic theory suggests that the monopoly team owner retains all rents. The theory is that factors of production (i.e., players) are paid according to their marginal product. In theory, therefore, a player will not benefit from a new stadium unless the new stadium positively alters the player's marginal product. The intent here is to examine whether conventional economic theory holds. Do players benefit financially from new stadiums after controlling for the impact that the stadium has on the player's marginal product? In other words, is rent sharing present? A key

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[1] The threat of re-location is not used in all cases. In some instances, the owners will make the case that the team can no longer be competitive if a new stadium is not built. This threat is expected to resonate with the local population that does not want to host a losing team.

[2] Note that other professional sports such as basketball and football do not enjoy the same immunity, as does baseball, but have nonetheless been successful in receiving public funds for stadiums. Their power likely relates to other (i.e., not officially recognized by the government) monopoly-like attributes of professional sports, for example, the high start-up costs for a competitive league to emerge. I am not aware of any research that indicates whether baseball is relatively more successful in obtaining public funds because of their immunity but the proliferation of new baseball stadiums in the 1990s makes baseball a particularly attractive case study.

element in answering this question is the ability to determine a player's marginal product. This will be done by following Scully (1974), who first set out a method for calculating the marginal revenue product of baseball players. Using ten years of data (1990-1999), empirical evidence suggests that baseball players, particularly pitchers, do in fact financially benefit from new stadiums, above and beyond what would be dictated by changes in their marginal productivity.

This research is unique in several respects. It is the first to examine the economic impact of stadiums on the players themselves, thus augmenting an already deep literature that has examined the impact of stadiums on team owners, local communities, and local politicians. It is also unique in how it addresses the question of rent sharing. For example, Rose (1987) and Black and Strahan (2001) each used industry de-regulation as the quasi-experimental event to find for rent sharing. That is, they find that payments to workers decrease as the industry moves from a monopolized state towards a more competitive state. Alternatively, this paper uses the construction of a new stadium as a quasi-experimental event, which instead represents a manifestation of the industry's monopoly power, as the industry capitalizes upon this power. In addition, the use of baseball enables a more precise way to control for marginal productivity. Whereas the other research is forced to use proxies for individual characteristics that influence worker productivity (e.g., education level), the ready availability of individual performance statistics within baseball enables a clearer distinction of the productivity of one player as compared to another. The findings in

[3] See Quirk and Fort (1992) for a nice exposition of various stadium lease agreements and a description of

this paper thus not only substantiate earlier findings for rent sharing, they have a stronger footing.

Chapter Three considers the case of defense spending. A rather rich literature has already examined whether the geographical distribution of defense spending is a function of political variables, particularly defense committee membership. Some authors such as Rundquist and a variety of co-authors[4] have in fact found support for the presence of distributive politics whereas other authors such as Mayer (1990, 1991) are critical of such findings.

In light of both these previous findings and the attendant criticism, the unique contribution of this research is two-fold. First, I aim to extend the previous empirical work of Rundquist and others to include additional years of data (i.e., extending the data series from 1963-1989 to 1963-2000) and modeling techniques (i.e., the inclusion of a fixed effects model) to see if previously found relationships continue to hold. Secondly, I develop an alternative model to respond to the criticism that has been lodged against models such as these. The primary criticism to which I respond is that the dependent variable is too broadly defined. While defense spending may be a homogenous grouping on the surface, a closer examination reveals a heterogeneous mix in which some subsets can be effectively targeted while others are less susceptible. In light of this, the second half of Chapter 3 focuses specifically on a subset of defense spending in

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the various ways in which owners benefit from these agreements.

[4] See, for example, Carsey and Rundquist (1999a, 1999b), Rundquist, Rhee, Lee, and Fox (1997) and Rundquist, Lee and Rhee (1996).

which the spending is homogenous and there is little ambiguity that the dollars can be targeted.

The results from the empirical work are provoking. In the first half of Chapter Three, I find that politically distributive trends which may have been present when analyzing data from 1963 to 1989 no longer hold when expanding the data series to include the 1990's, a decade synonymous with a massive drawdown in defense spending. In the 1990s, the House apparently loses power to direct contracts to home districts whereas the Senate appears to gain power. Several reasons are explored for this. First, there may have been a fundamental institutional shift. It may be the case that the drawdown of the 1990s inflicted a disproportional hit on representatives who had previously benefited from pork-barrel type projects. It is also possible that the way in which Congress managed the drawdown created a power shift from House to Senate. Alternatively, it may be the case that the findings here and in closely related literature are spurious to some degree, owing in part to the use of a dependent variable, as mentioned above, that is too highly aggregated.

The second half of Chapter Three thus focuses on a subset of the defense budget that can be evaluated at a much more granular level. Again, the results are provoking. Using data from military construction earmarks for the 104^{th} through 106^{th} Congresses, I show there is a statistically significant political influence in military construction spending. This result, itself, is not altogether surprising considering the fact that the data is gleaned from the appropriations earmark process, which is inherently political. The sub-plots, however, are revealing. For instance, seniority appears to play little function in the distribution of military construction earmarks. There also appears to be a tight coupling between close elections and earmark targeting. Other findings are discussed Chapter Three.

Chapter Four will conclude this research with a summary of the findings. In addition, I will discuss possible directions for follow-on research. For example, it would be interesting, among other things, to see if the findings for rent sharing in baseball extend to other professional sports. In the defense work, there would be value in examining political influences in the military construction budget that was determined prior to the addition of earmarks resulting from the appropriations process in Congress.[5]

The study of the economics of baseball and defense spending is an odd coupling on the surface. Yet, each area is particularly conducive to a study involving the "division of spoils" because data is available for each at a granular level, yet can be gathered systematically on a large-scale basis. More importantly, the study of each sheds light on a myriad of questions involving the forces that influence the distribution of money throughout the economy. Considered against the backdrop of the existing literature, this research both substantiates earlier findings and sheds light on others. For example, the finding for rent sharing in baseball confirms similar findings in the trucking and banking industries. The study of military construction earmarks, an idea prompted by the study of earmarks in academics, breaks new ground in the study of the distribution of dollars within defense. So, despite the disparate nature of the two subjects, each provides a unique venue to improve and uniquely contribute to the collective body of literature on the "division of spoils".

[5] Note that this research focuses solely on the part of the military construction budget determined by the earmarks.

Chapter 2: The Returns to Major League Baseball Players from New Stadiums

The goal of this chapter is two-fold. First, it seeks to fill a gap in the literature on the public financing of professional sports venues. The literature has not evaluated the economic interest of the professional sports athlete as it relates to the construction of new athletic venues. Accordingly, this chapter will seek to examine and quantify the benefits (i.e., increased salary) that professional baseball players receive when playing on a team with a new stadium. Secondly, this chapter seeks to augment the burgeoning literature on testing for rent sharing from monopoly owner to labor. In testing for rent sharing, baseball offers a unique opportunity to control for individual performance that is not present in otherwise similar studies.[6] The findings will be discussed in terms of implications for the larger body of economic theory.

Section 2-1 introduces the nature of the problem. Section 2-2 provides a literature review. Section 2-3 describes the model to be tested. Section 2-4 describes the data. Section 2-5 presents the results. Section 2-6 concludes.

2-1: Introduction

The public financing of professional sports venues has generated a rich literature. The literature has focused on both explaining why local communities might subsidize

[6] As discussed in Chapter One, this paper considers a team's ability to extract generous public funding arrangements for new stadiums as one sign of its monopoly power and thus uses the construction of a new stadium as a quasi-experimental event to test for rent sharing.

sports facility construction and quantifying the benefits that local communities receive and the costs they incur. As for why public subsidies are provided, Noll and Zimbalist (1997) offer a concise summary.[7] Communities provide public funding for sports facilities because: 1) they may generate substantial regional psychic benefits; 2) they may stimulate regional financial growth; 3) cities gain federal tax benefits when a facility is publicly constructed; and 4) incentives of the political system may favor such funding.[8]

Economic analyses have followed which examine the benefits and costs of various facilities. A majority of the analysis suggests that the public costs outweigh the benefits. It is often pointed out, however, that benefits such as civic pride in a team and stadium is difficult to quantify as a tangible benefit. Fort and Quirk (1992) state, "It might well be that the most important benefit that a team provides for a city is as a common identification symbol, something that brings the citizens of a city together…"[9]

Community activists, with a focus on the economic analyses that suggest stadium benefits are exaggerated, have generated normative literature that decries the perceived abuse of public funds when other opportunities for this money exist. Frequently, the targets of these normative attacks are the perceived greedy owners of sports teams that request public financing and the perceived less-than-honorable politicians that endorse public financing of new venues.

Conversely, economists in the Public Choice school have examined the political and economic incentives that foster political support for public financing of sport

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[7] Noll and Zimbalist (1997), 25-26

[8] For instance, a politician may face a high payoff if a stadium is approved or may be dealt a severe blow if professional sports team relocated if a stadium is not provided.

[9] Fort and Quirk (1992), 176

facilities. They suggest that team owners and local politicians are no more profit-driven or self-serving than any others are. Instead, they suggest it is the incentive structure of the political system that aligns the interests of politicians and team owners to form coalitions that seek the use of public funds for stadiums over alternative and perhaps more beneficial or appropriate uses.

Regardless, there is a constant in these approaches. Each has focused on either one element of this triangular network (i.e., team owner, local politician, or local community) or on the relationships that link the three. One key factor, however, is missing namely the professional athlete who plays in these venues. Once considered, questions emerge: Do players materially benefit when a team receives a new venue? If so, what is the magnitude and composition of this benefit? How does it impact the dynamics of venue financing and construction?

Economic theory would suggest that players benefit from playing in a new venue because a venue is a complementary capital input that positively alters the players' marginal revenue product (MRP). When a venue is built, it is a community novelty. The construction of a baseball stadium in Baltimore in 1992, for example, ushered in a wave of "retro" stadiums. These stadiums were built to have the charm of old-time ballparks combined with modern luxuries and they have been a commercial success for the past decade as a result. The demand for baseball is greater. That is, the demand curve has shifted outwards, causing revenues to increase. If players and stadiums are defined as complementary inputs in the production of sports entertainment, the value of the players in theory, increases. The value of the players would also increase if the new stadium improves the player's on-field performance, assuming that improved performance translates into an increased revenue flow. Accordingly, athletes have a vested interest in new venues. Furthermore, they would have a relative preference for publicly financed venues over privately financed ones, assuming that a new venues has a higher probability of being built if publicly funded.

A second, and perhaps esoteric reason, why a player's salary might increase from playing in a new stadium is rent sharing from team owners to the players. The case is often made that a professional sports team owner is a monopoly provider of professional sports to a local community. Using this power, owners have successfully obtained public funding and generous lease arrangements for new facilities. They use the credible threat that either the team will re-locate to another city if a new venue is not provided or that the team will not be able to compete with teams that have new stadiums or otherwise greater local revenue. Either generous lease terms or the provision of public funds for stadium construction can then be considered an economic rent to the team owner. At question is whether professional players are co-claimants of these rents.

It is critical to point out here that while new stadiums are a complementary input with players in the baseball "production function", the stadium also can be considered an independent factor in the baseball-going experience. In other words, some fans are attracted to baseball games not to see a particular player but to enjoy the ambience of the stadium apart from the players. In economic terms, the marginal revenue product of the player is not impacted. From a conventional theory perspective, therefore, players would not benefit if the new stadium does not impact their marginal revenue product, unless rent sharing between owner and player is indeed present.

Thus, at the heart of this chapter, are two questions to be explored empirically:

1. Do professional athletes benefit from new venues?

2. If so, what is the source of this benefit? Do salaries increase from rent sharing or because a new stadium impacts a player's marginal revenue product? Are both factors at play?

This line of investigation is unique. First, as previously mentioned, the economic literature on sports facilities, while deep, has not included the interests of the participating athletes. This chapter will expand the literature and cast a new light on the economics and politics of sports facilities, by including the athlete. Second, the literature on testing for rent sharing is relatively small. A handful of studies have found evidence of rent sharing in other industries. The sports industry, however, offers a particularly fertile ground for empirical testing because it offers a wealth of individual performance statistics and salary information that is not a luxury for studies of other industries. The ability to control for individual performance strengthens the empirical examination of rent sharing hypotheses. Note that the specific focus of this chapter within the sports domain will be on professional baseball since baseball offers particularly deep performance and financial information and has witnessed a boom in new stadium construction over the past decade.

2-2: Literature Review

The research questions introduced above cut across several strands of literature. First is the research on the benefits and costs of new stadiums. This literature has not yet examined the interests of the professional athlete as these interests relate to the dynamics of sports stadium financing, which is a goal of this paper. It is instructive, however, to summarize the stadium literature to show the depth of research in the other facets. The second strand of literature centers on the best specification for an equation of individual salary determination in baseball. A salary determination model is vital to the questions posed here. The goal is to determine if new stadiums have a positive and significant impact on player salary, controlling for other widely accepted determinants of player salary. Scully's seminal article in 1974 provides such a model and has been followed up by both Scully and others and this offers a foundation for the salary specification model to be employed here. Closely related is the literature on methods to compute the marginal revenue product (MRP) of baseball teams and players. One goal in this chapter is to distinguish between marginal productivity and rent sharing effects on player salary. A method of MRP calculation that is consistent with the existing literature is therefore essential. The final strand of literature involves rent sharing. To this point, rent sharing literature has focused on other industries. Yet, this literature is quite germane as a pre-cursor to examining rent sharing in sports. Each strand is discussed below.

2-2.1: Sports Facilities

The research of the economics and politics of sports venues is extensive. Most has focused on the costs and benefits of venues to team owners, local communities, and local politicians. As such, it is not completely relevant to the unique question of whether there is a venue effect on athletes' salaries. For reference purposes, however, the stadium literature is briefly summarized.

For a thorough overview of sports facilities to include history, naming rights, financing details, and current status, the reader is referred to Anderson's (2001) *Sports Facility Reports* published periodically by the Marquette University Law School. With respect to cost-benefit type studies, the reader is referred to Coates and Humphreys (2000), Jensen (2000), Campbell (1999), Noll and Zimbalist (1997), Baade (1996), Coffin (1996) Rosentraub, Swindell, Przybylski, and Mullins (1994), Quirk and Fort (1992), and Baade and Dye (1990). For the normative perspective of stadiums from community activists, refer to Cagan and deMause (1999). For a Public Choice perspective of the approval and financing of stadiums, the reader is referred to O'Roark (2001), Tollison (2001), Fort (2000), Rich (2000), Fort (1999), Fort (1997), and Safir (1997).

Since this literature has not examined the impact of new venues on player salaries, the economic modeling techniques used in the above works are not germane. The goal here is to augment this literature, but by employing approaches from other strands of literature described below.

2-2.2: Specification of a Model for Player Salary

Scully (1974) hypothesizes that individual player salary is a function of player performance as measured by several batting or pitching statistics, fan interest, and community population characteristics. He finds that "...the salary process in major league baseball is quite deterministic. The concept that ballplayer salaries are related to performance seems reasonably well confirmed."[10] Scully finds that non-performance factors such as regional per capita income have little explanatory power while the intensity of fan interest has a positive effect on player salary. Scully has updated his work numerous times including, for example, Scully (1995) in which he makes several refinements. For example, he cites total bases[11] as the single best indicator of batter performance.[12] He also suggests that cumulative games played is the best variable in controlling for player experience as compared to using cumulative years of experience since effort expended can vary dramatically from year to year. He also recommends adding the square of the experience variable to capture the hypothesized diminishing returns for experience. Finally, he proposes that lagged values for each independent variable be used since salary is determined before a season starts and salary is best considered a function of previous season performance.

Fort and Quirk (1997) point out that attempts to update the Scully model "have all reached the same conclusion: the variables are so highly correlated that once a few are included, more variables provide little added explanation."[13] Nevertheless, they suggest refinements to account for oversights in the Scully model. Their concerns involve how different types of players may be differentially emphasized by the independent performance variables selected. For example, innings pitched as a way to capture a pitcher's experience will result in over-emphasizing starting pitchers as compared to

[10] Scully (1974), 915

[11] Total bases is a weighted score of hits. Each single is counted as one; each double as two, and so on. In the 1974 article he used slugging average, which is total bases divided by times at bat.

[12] Scully (1995), 50

relief pitchers. They also point out there are nonlinear effects with respect to age and experience. They introduce technical adjustments to account for these and related concerns.[14] They use their refined model to examine whether the returns to performance increase from the era when the baseball labor market was restricted (prior to 1976) to the current era where players gain free agency in their sixth year. They conclude that an increase in the magnitude in each of the coefficients in the salary equations during the current free agent era reflect both the greater bargaining strength of the players from free agency and an overall increased demand for baseball.

These various models provide insight into the most important determinants of salary. This, in turn, has facilitated the comparison of salary determinants between different periods. For example, is there a tightening of the link between pay and performance as the players have gained more power in the labor market through free agency? The particular purpose in this paper is to see whether new venues have an impact on player salary. One way to test for this, as will be discussed in the modeling section of this chapter, is to add a dummy variable to some form of the Scully salary model and to test for the sign and significance of the stadium variable.

2-2.3: Marginal Revenue Product in Baseball

One factor that the above salary models do not explicitly address is a player's marginal revenue product (MRP). In the theory of competitive labor markets, a laborer is paid according to MRP, no more and no less. If this were the case in baseball, then salary

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[13] Fort and Quirk (1997), 369-370

and MRP could be used interchangeably in the preceding models. In reality, this equivalency between salary and MRP likely does not hold on a one-for-one basis. For example, baseball owners are the sole buyer of baseball playing talent in the market, giving them leverage over the players. In theory, this would depress salary below MRP. Nonetheless, if MRP can be defined, there is value in doing so. Again, an objective of this chapter is to distinguish between rent sharing and MRP movements in player salary. A review of the literature on calculating MRP within baseball is thus considered here.

Scully's previously mentioned classic piece in the *American Economic Review* dealt with developing a method for determining MRP. His goal was to compare individual player MRP with the player's salary to measure the degree of player exploitation in what was then a largely restricted labor market. In other words, Scully hypothesized that salary was significantly below MRP. Scully found for instance, that star players were paid roughly 20% of their MRP.

Scully's calculation of individual MRP consists of three steps. His approach follows the standard economic definition of MRP as being the product of marginal revenue (MR) and marginal product (MP). Scully's first two steps thus involve proposing a team revenue function and a team production function. With these functions specified, he can compute team marginal revenue and marginal product, and then multiply them to compute a team MRP. In the third step, individual player MRP is

[14] Fort and Quirk do an excellent job of presenting their statistical approach in an appendix of their book *Pay Dirt*. The approach of this paper will stay closer to the original parsimonious approach of Scully since the nuances introduced by Fort and Quirk are not as critical for the question being pursued in this paper.

calculated from the team MRP based on that individual's contribution to the team's performance.

In the first step, Scully defines a team's total revenue function. He says that total revenue consists of gate (attendance) revenue and media revenue. He then hypothesizes that this revenue is a function of the team's success on the field as measured by the team's winning percentage, population of the metropolitan area, and several other team characteristics. A simplified version is provided:

(Eq. 2.1) $TR_j = \alpha_0 + \alpha_1(pop_j) + \alpha_2(winpct_j) + \alpha_3(otherteam_j)$ where:

TR is team total revenue of team j

pop is the metropolitan area population for team j

winpct is the team's winning percentage

otherteam is a vector of other team characteristics including for instance a dummy variable if the team is in the National League, versus the American League

The basic idea of equation 2.1 is that a team will generate some amount of revenue simply by virtue of its market size, independent of team performance. This is captured by the coefficient on regional population. The marginal impact of team performance on revenue is by way of the team's on-field success, which in turn influences, for example, how many people attend the games or demand the games on television. In order to calculate return to team performance, Scully links the revenue function in equation 2.1 to team performance measures by way of winning percentage. In other words, he hypothesizes that the team's winning percentage (*winpct*), which is an explanatory variable in equation 2.1, is itself a function of offensive and pitching performance.

For a measure of offensive performance, he uses slugging average (*slug*) as the best explanatory variable. For pitching, he uses the ratio of strikeouts to walks (*ratio*) as the best explanatory variable.. This yields the following equation for winning percentage:

(Eq. 2.2) $winpct_j = \beta_0 + \beta_1(slug_j) + \beta_2(ratio_j)$:

A team's MRP with respect to offensive performance is then obtained by multiplying the marginal revenue of winning percentage from equation 2.1 and the marginal product of team batting from equation 2.2. Specifically, equation 2.3 shows how revenue responds to a one-unit increase in the team's offensive performance.

(Eq. 2.3) $$\frac{d(TR_j)}{d(slug_j)} = \frac{d(TR_j)}{d(winpct_j)} * \frac{d(winpct_j)}{d(slug_j)} = \alpha_2 * \beta_1 = MRPOFF_l$$

The marginal return for a one-unit increase in pitching performance ($MRPPITCH_l$) is similarly computed. Note that the measure that arises from these calculations is a league-wide measure.[15] Scully uses information for all teams across a short time span to calculate one figure as opposed to computing a specific measure for each team.

To compute individual player MRP from this single factor, Scully makes simplifying assumptions about a team's roster. He points out that on a given roster there will be twelve regular offensive players so each player represents 1/12th of the offensive

[15] The subscripted l for each of the two measures stands for league.

roster. There are eight regular pitchers so each pitcher represents 1/8th of the pitching roster. Individual MRP is then computed as follows:

(Eq. 2.4) $MRPOFF_i = \frac{1}{12} * slug_i * MRPOFF_l$

(Eq. 2.5) $MRPPITCH_i = \frac{1}{8} * ratio_i * MRPPITCH_l$

where the MRP of any individual player *i* is factored from the league (*l*) derived MRP for teams. Scully points out that, "Since the production function omits a number of inputs, the marginal products of players may be overstated."[16] To account for this, Scully deducts certain costs from total revenue in order to compute a net marginal revenue product. Even after factoring this in, Scully finds that players are paid well below their MRP.

Scully's work in 1974 has generated a comprehensive follow-on literature, much of which is focused on using variations of the Scully model to find for exploitation and discrimination effects in player salaries. Zimbalist (1992) makes four modifications to Scully's proposed calculations for MRP. First, he points out that Scully makes a counterfactual claim for batters of no hits. That is, "he assumes that if the player being evaluated did not play, then his replacement would have been up the same number of times and had no hits."[17] In this case, a player's estimated marginal production will be overstated and perhaps substantially so. Zimbalist's alternative compares the performance of the team with the player and without the player. Secondly, he uses different measures of offensive and pitching performance. For offense, he uses a

[16] Scully (1995), 921.

combination of slugging average[18] and on-base average because it gives greater weight to players who are able to get on base but may not necessarily hit with much power. For pitching, he uses earned run average instead of the strikeouts to walk ratio. Third, he uses winning percentage lagged one year in addition to current year winning percentage in the equation for team revenue. Finally, he includes an alternative procedure to account for net marginal revenue product. Zimbalist finds that younger players who are not able to move freely in the labor market are exploited. Players who have gained free agency status in their sixth year (a substantial change in the labor system from when Scully first wrote) tend to earn, if not exceed, MRP.[19]

Fort and Quirk (1996) argue that the estimated exploitation of players by monopsonistic owners is exaggerated by as much as 16% because research has failed to account for revenue sharing among teams. They develop a model to account for revenue sharing arrangements that prevail in the league. These arrangements have a dampening effect on player salaries in addition to the dampening effects that monopsony has on salary.

Krautmann (2000) claims that MRP estimates generated by the Scully model are exaggerated. He says that the Scully method of apportioning revenue to individual players based on such things as the individual's percentage of the team's at-bats systematically over-estimates the player's marginal value and ignores the effect of non-

[17] Zimbalist (1992), 188
[18] This is the statistic that Scully used.
[19] In theory, salary would not exceed MRP but various theories have been forth as to why this may be the case, to include the fact that MRP calculations are, as Scully pointed out, crude.

player inputs such as coaches.[20] Instead, Krautmann makes the assumption that players who actually gain free agency status and negotiate a competitive contract will obtain a salary that approximates MRP.[21] He then regresses the salary of actual free agents (which equals MRP by his assumption) on the player's performance measures. He takes the coefficients for the performance variables from this regression and applies them to players who have yet to gain free agency. By so doing, he imputes the true value of these players to their respective teams. He finds that Scully estimates of MRP, and thus exploitation, are over five times higher than those generated by this method. Krautmann's work focuses primarily on batters as he claims that the wide variety of the types of pitchers (e.g., starting pitcher, middle reliever, and closer) makes it difficult to find a single measure of performance to impute a pitcher's monetary value to a team.

One criticism that can be lodged against each paper is that estimates of individual marginal revenue product are, to varying degrees, crude.[22] Scully points out that the revenue and production functions in equations 2.1 and 2.2 cannot be examined with the individual player as the unit of observation because fans cannot discriminate through attendance on the basis of player appearance since regular players play in virtually every game. Evaluating pitchers at the individual unit of observation would be possible since pitchers play in only a fraction of games. Thus, one could compare attendance when a particular pitcher plays and when he doesn't. The ability to do this, however, would be

[20] One way to account for this omitted variable bias is by using a fixed effects model. This paper will consider this possibility.
[21] Krautmann's assumption that a free agent's salary equals MRP is perhaps too much of a stretch. Despite a relatively free labor market, players still face monopsonistic team owners, which would tend to dampen salary below MRP. In this case, Krautmann's estimates are likely too low.
[22] A criticism that Scully (1974) readily acknowledges on page 918.

difficult since this type of data is not systematically collected on a large-scale basis. Another criticism is that the Scully-based process for MRP calculation assumes that the production function of the players is that of winning games, as opposed to being more broadly defined to be that of providing entertainment. For example, certain star players may attract fans to the park, independent of team or individual on-field performance. As mentioned above, however, it is virtually impossible to distinguish a player's marginal impact in drawing fans to the park if the player plays in most if not all of the team's games. It can also be countered that such star players are rare. Furthermore, being a star player typically implies that the player is a strong on-field performer, which Scully-based models capture to a great extent.

2-2.4: Rent sharing

The final strand of relevant literature involves rent sharing itself. The seminal paper is Rose's 1987 study of rent sharing in the trucking industry. Rose found that labor is an important claimant to firm's rents. Rose used labor deregulation within the trucking industry in the late 1970s as a natural experiment and found empirical support for the rent sharing hypothesis. She found that the premia for union membership fell from 50 to 30 percent after deregulation. She estimated that the union captured over two-thirds of the total rents but did not find any spillover effects for either non-union members or for those in previously non-regulated industries.

Blanchflower et al (1996) examined several industries. Controlling for industry fixed effects and a range of personal and compositional variables, they found that worker pay follows earlier movements in industry profits thus again lending support for a rent sharing hypothesis.

Black and Strahan (2001) studied the banking industry and in particular used banking deregulation as a natural experiment to study wages prior and post deregulation. They find that non-unions share in industry rents, but that unions receive even higher returns. They also show that after deregulation, wage cuts fell disproportionately on men suggesting that women were discriminated against prior to deregulation. This supports Becker's theory that wage discrimination is less likely to occur in a free market.

Each of these studies controls for individual characteristics that may influence salary to some degree. They do this in order to separate the effects of rent sharing and other individual characteristics that may impact that person's pay. At best, however, these studies are only able to control for such things as individual education level and years of experience in the particular field. The studies are not able to go much further than that. As Black and Strahan (2001) point out, attempts to measure rent sharing effects have been elusive because "...of the difficulty of isolating rents paid to workers from compensating wage differentials, payments to unobserved human capital, and efficiency wages."[23] As such, it is possible that the various findings for rent sharing may be spurious, resulting from omitted variable effects that the researchers are not able to effectively control.[24] This chapter's focus on sports uniquely facilitates the control of

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[23] Black and Strahan (2001), 814.

[24] This shortcoming is acknowledged and various approaches are used to circumvent it. For example, Black and Strahan (2001) indicate that decreased payments to workers after deregulation might point to the fact that people with a lower skill set are employed; however, they find that the skills within the banking industry do not change after deregulation lending credence to their finding of rent sharing.

individual performance to a degree not available in other studies, such that any findings for or against rent sharing are more robust.

2-3: Empirical Models

The models presented in this section are aimed at addressing two questions. First, what are the gross returns to a player from playing in a new stadium? Two different salary specification models are employed and each includes a dummy variable for new stadiums to capture gross stadium effects. Second, if these returns exist, what is the source? Do changes in a player's salary result because the new stadium has increased the player's marginal revenue product or because new stadium rents are being shared? These two questions will be addressed in turn.

2-3.1: Estimating Gross Returns to New Stadiums

To what extent do players benefit from playing in a new stadium? This paper will suggest two specifications for a salary model to answer this question. The first model keeps with the Scully tradition described in the literature review. It will be referred to as the "classic model" for salary specification. It is described in equation 2.6 below:

(Eq 2.6) $$\begin{aligned} salary_{i,t} &= \alpha_0 + \alpha_1(\exp_{i,t-1}) + \alpha_2(\exp^2{}_{i,t-1}) + \alpha_3(perf_{i,t-1}) + \\ &\alpha_4(newstad_{i,t-1}) + \mu_{it} + \varepsilon_i \end{aligned}$$, where:

salary is the log of player i's salary at time t

exp is player i's experience as measured by cumulative career at bats at t-1 or innings pitched for pitchers

perf is player i's previous years performance. For batters, total bases are used. For pitchers, earned run average is used.

newstad is a dummy variable=1 if player played in new stadium the previous year

μ represents the normal error term

ε represents the unknown but constant parameter to be estimated for each player

Following Krautmann (2000), Scully (1995), and others, the lagged values for both the performance and experience variables are used since salary is determined before a season starts and is more a function of previous year's performance and experience than of current year. The new stadium dummy variable is lagged to reflect the idea that owners wait to see what revenues are generated in a new stadium before passing that on to the players.[25]

The sign on the lagged performance variable for batters is expected to be positive since an extra total base generated reflects greater productivity. The sign on the lagged performance variable for pitchers is expected to be negative since a lower earned run average (ERA), which is a measure of the number of runs a pitcher allows when pitching, usually signals a higher quality pitcher.[26] The sign on experience is expected to be positive; however, there are diminishing returns to experience such that the square of experience would carry a negative sign.

The proposed inclusion of the fixed effect constant term eliminates bias in the independent variables resulting from omitted variables. For example, if the player were a star defensive player throughout his career, this would be captured by the unknown but constant term since the right hand side variables do not include measures of defensive performance. The inclusion of the constant term is inefficient because a separate term is

[25] If a player played in a new stadium at time t-1, but then was traded to a team at time t that has an old stadium, the observation will be dropped

estimated for each player so that only variation within the unit of observation (in this case the player) is considered. In this chapter, the results will be reported when the individual salary model is run with and without the constant term.

The variable of particular interest in Equation 2.6 is the dummy variable for new stadiums (*newstad)*. If this variable carries a positive sign then players in fact benefit from playing in a new stadium, having controlled for relevant performance and experience variables. Note this is a measure of the gross benefit of new stadiums, which is of interest itself. The following section will address ways in which the sources of this benefit, if it exists, can be identified.

One nuance of the stadium variable is how long a stadium is considered new. Coffin (1996) suggests four years and this will be considered the baseline case for this study. Alternatives, however, will be considered. Stadiums in Baltimore and Cleveland are still considered novel though they both are about ten years old. Two alternative regressions will consider a stadium new if less than seven years old and also if less than ten years old to determine the sensitivity of the parameters to this length.[27]

The above model keeps to the spirit of Scully's (1974) classic model. Quirk and Fort (1997) point out that once additional performance variables are added to the right-hand side, little additional explanatory power is gained. The risk is the threat of multicollinearity, which masks the measures of marginal effects. A possible objection to the classic salary model of equation 2.6 is that the experience variable is continuous

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[26] Although not used for batters, a dummy variable for league (A.L. or N.L.) may be required for pitchers since A.L. pitchers systematically have higher ERAs because they face the designated hitter.

[27] To maintain the focus of this chapter, however, the alternatives for stadium age will be considered in Appendix 2.2.

suggesting smooth increases in player salary as experience increases. An alternative specification may better account for the nature of baseball's labor market in that there are discrete points in time at which a player's salary can be expected to jump non-linearly. Specifically, players with six years of experience or more are eligible to become free agents such that teams can competitively bid for their services. Players with fewer than two years have no power to market their services while players in the 3-5 year range can submit to salary arbitration giving them some power in relation to team management, but not as much power if they had free agency status. This would suggest three distinct categories of player classification (rookie, arbitration eligible, and free agent eligible). There may also be non-linear returns for superstar players. Considering this potential non-linearity, I propose in equation 2.7 an alternative specification to the classic model that eliminates the experience variables of equation 2.6 and replaces them with indicator variables for whether the player is free agent eligible, a rookie as well as a star:

(Eq. 2.7) $$\begin{aligned} &salary_{i,t} = \alpha_0 + \alpha_1(perf_{i,t-1}) + \alpha_2(free_{i,t-1}) + \alpha_3(rook_{i,t}) + \alpha_4(star_{i,t-1}) + \\ &\alpha_5(newstad_{i,t-1}) + \mu_{it} + \varepsilon_i \end{aligned}$$, where:

salary, perf, newstad, $\mu_{i,t,}$ and ε_i are as defined in equation 2.3 above

free is a 0-1 dummy variable, with value of 1 if player had six or more years of experience at t-1

rook is a 0-1 dummy variable with value 1 if player has less than two years of experience[28]

star is a 0-1 dummy variable with value 1 if player has less than two years of experience at t-1

[28] This variable is not lagged since rookie status figures into salary immediately.

This model will be referred to as the "alternative model" of salary specification. The coefficient for *free* is expected to be positive to account for the increased bargaining power a player gains by having free agency status. Conversely, the coefficient for *rook* is expected to be negative since rookie player movement is limited. The coefficient for *star* is expected to be positive since there is value in being an all-star player. Finally, the coefficient on *newstad* is expected to be positive as is hypothesized for equation 2.6.

Again, the model will be run with and without a player-specific fixed effect. Relative to the classic model in equation 2.6, the potential for an omitted variable bias rises in this alternative model since the experience variable, which is a player-specific measure in the classic model is replaced by the more general labor experience dummy variable classifications in the alternative model. The case for employing a fixed effect model is thus relatively stronger when considering the alternative model.

2-3.2: Distinguishing the Source of New Stadium Benefits on Player Salary

The coefficients on the new stadium variable in equations 2.6 and 2.7 are gross measures of the benefit that players receive from a new stadium. While this gross measure is of interest in its own right, there is no distinction as to whether the benefit, if any, is driven by a change in the player's marginal productivity in the stadium or if it results from rent sharing. I propose two ways to isolate these sources of change. The first is described in equations 2.8 and 2.9, obtained by slightly modifying equations 2.6 (classic salary model) and 2.7 (alternative salary model).

(Eq. 2.8) $$\begin{aligned} salary_{i,t} &= \alpha_0 + \alpha_1(\exp_{i,t-1}) + \alpha_2(\exp^2{}_{i,t-1}) + \alpha_3(perf_{i,t-1}) + \alpha_4(newstad_{i,t-1}) + \\ &\alpha_5(perf_{i,t-1} * newstad_{i,t-1}) + \mu_{it} \end{aligned} \quad ,:$$

(Eq. 2.9) $$\begin{aligned} salary_{i,t} &= \alpha_0 + \alpha_1(perf_{i,t-1}) + \alpha_2(free_{i,t-1}) + \alpha_3(rook_{i,t-1}) + \alpha_4(star_{i,t-1}) + \\ &\alpha_5(newstad_{i,t-1}) + \alpha_6(perf_{i,t-1} * newstad_{i,t-1}) + \mu_{it} \end{aligned},$$

where all variables are as described before with the sole addition of an interaction term for performance and new stadium. The coefficient on this interaction term measures the value to the player of a marginal increase in performance specifically when playing in the new stadium. Assuming that MRP is closely correlated to salary, the interaction term is one way to isolate the effect that the new stadium has on the player's marginal product. The dummy variable for new stadiums in each equation is then left to capture all residual effects that the new stadium has on salary to include, for example, rent-sharing effects. If this coefficient is positive, this points to a finding for rent sharing.

While this is one method to separate MRP and rent-sharing attempts on salary, the above method does not explicitly use a measure of individual player MRP.[29] The second approach used to distinguish rent-sharing and MRP effects on salary is to calculate individual player MRP using the Scully framework described in the literature review. A regression of individual salary on MRP, experience, and on a dummy variable for new stadiums then isolates the two effects of interest. If rent sharing were present, the coefficient on the new stadium dummy variable would be positive, controlling for individual marginal revenue product.

In the literature review, Scully's 1974 method for determining player marginal revenue product was detailed in equations 2.1 through 2.5. Specifically, in equations 2.1 through 2.3, he examines a team revenue and production function. He posits that team revenue is a function of regional population, team success, and other team characteristics. In equation 2.3, he shows how team revenue responds to team on-field performance and thus provides a measure of marginal revenue product (MRP) at the team level. In equations 2.4 and 2.5, he then explains how individual MRP is computed by virtue of an individual player's contribution to team success. The equations that are described next provide a modified form of the Scully approach.

Equation 2.10 below follows Scully's (1974) team revenue equation described in equation 2.1, but is modified primarily to include a dummy variable for new stadiums.

$$\begin{aligned} TR_{jt} &= \alpha_0 + \alpha_1(pop_{jt}) + \alpha_2(winpct_{jt}) + \alpha_3(winpct_{j,t-1}) + \alpha_4(newstad_{j,t}) \\ &+ \alpha_5(season_t) + \mu_{jt} + \varepsilon_j \end{aligned} \qquad \text{(Eq. 2.10)}$$

where:

TR is team total revenue of team j in year t

pop is the metropolitan area population for team j in year t

winpct is the team j's winning percentage in year t

newstad is a binary variable that equals one if the team is playing in a stadium that is 4 years old or younger at time t and equals zero otherwise

season dummies are included to account for season-specific changes in revenue

μ represents the normal error term

ε represents the unknown but constant parameter to be estimated for each team

The principal difference between equation 2.10 above and Scully's original model described in equation 2.1 is the inclusion of a dummy variable for teams with new

[29] The assumption that salary approximates MRP must be made to make any conclusions from the regressions in equations 2.8 and 2.9.

stadiums. A second difference is the inclusion of a lagged value of winning percentage. Following Zimbalist (1992) and Krautmann (2000), lagged winning percentage will be included in addition to current season winning percentage to account for the effect that last year's performance has on current year revenue.[30] A third difference is the inclusion of a fixed-effect constant term. The inclusion of this term reduces the bias in the independent variables that may result from omitted variables, such as other fixed characteristics of the team that cannot be quantitatively captured but which nonetheless influence team revenue in a constant manner over time.

In equation 2.10, the team's winning percentage is the variable that links team revenue with team on-field performance. As shown in equation 2.11 below, which follows Scully's equation 2.2, winning percentage is hypothesized to be a function of pitching and batting performance. Equation 2.11 is modified from equation 2.2, however, to include different measures of team offensive and pitching performance. It also includes interaction terms of the stadium and team performance for both offense and pitching.

$$\text{(Eq. 2.11)} \quad \begin{aligned} &winpct_{j,t} = \beta_0 + \beta_1(totalbases_{j,t}) + \beta_2(era_{j,t}) + \beta_3(totalbases_{jt} * newstad_{jt}) \\ &+ \beta_4(era_{j,t} * newstad_{j,t}) + \mu_{j,t} + \varepsilon_j \end{aligned} :$$

In equation 2.11, I use the number of a team's total bases, following most recently Krautmann's (2000) use of total bases. For pitching performance, I use earned run average (*era*), again following most recently Krautmann (2000). The two interaction terms in equation 2.11 are of particular interest here as they measure how the new

[30] Krautmann and Zimbalist deflate the importance of lagged winning percentage by discounting it

stadium alters the winning percentage of a team in a new stadium through the stadium's impact on a team's ability to produce runs or pitch better. If the coefficient on the interaction variable for batting (that is β_3) is positive for instance that means the new stadium has a positive impact on team run production and thus on winning percentage. Winning percentage in turn is hypothesized to have a positive impact on team revenue in Equation 2.10. The marginal revenue product calculation of the team performance variables for batters and pitchers are provided in Equations 2.12 and 2.13 using information from equations 2.10 and 2.11 respectively:

(Eq. 2.12)

$$\frac{d(TR_{j,t})}{d(totalbases_{j,t})} = \frac{d(TR_{j,t})}{d(winpct_{j,t})} * \frac{d(winpct_{j,t})}{d(totalbases_{j,t})} = (\alpha_2 + \frac{\alpha_3}{1.07}) * (\beta_1 + \beta_3 * newstad_{j,t})$$

(Eq. 2.13) $$\frac{d(TR_{j,t})}{d(era_{j,t})} = \frac{d(TR_{j,t})}{d(winpct_{j,t})} * \frac{d(winpct_{j,t})}{d(era_{j,t})} = (\alpha_2 + \frac{\alpha_3}{1.07}) * (\beta_2 + \beta_4 * newstad_{j,t})$$

Equations 2.12 and 2.13 show how a new stadium alters the MRP of batters and pitchers respectively. If a new stadium positively alters team MRP then either β_3 or β_4 will be significant, assuming that α_2 is positive and significant. Note that these coefficients would respectively be positive for batters or negative for pitchers since a lower earned run average (era) for a pitcher signals better performance. A new stadium also impacts team revenue apart from the team's winning percentage. This impact is measured by the coefficient α_4 in equation 2.10.

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approximately seven percent.

The express purpose of the preceding process is to isolate the two effects of a new stadium on team revenue. The products of $\alpha_2 * \beta_3$ and $\alpha_2 * \beta_4$ from equations 2.12 and 2.13 capture how a new stadium marginally improves the team's revenue by way of the stadium's impact on the team's batting and pitching and thus on the team's winning percentage. The coefficient α_4 from equation 2.10 captures all other effects that a new stadium has on team revenue outside of the stadium's effect on the team's winning percentage.

In theory, the collective group of players could claim an increased salary only to the extent that the stadium has collectively enhanced the team's MRP, as captured by $\alpha_2 * \beta_3$ and $\alpha_2 * \beta_4$. The players could not lay claim to the increased revenue from stadiums as measured by α_4 because this coefficient captures the marginal returns to a new stadium, outside of the new stadium's effect on winning percentage. The players would only benefit in this latter case if team owners shared these rents with them by way of some other forcing mechanism.

The above looks at this question from the general team level. One way in which to view this at the individual level is to compute individual player marginal revenue product and substitute this MRP calculation into the individual salary models detailed in equations 2.6 (classic salary model) and equation 2.7 (alternative salary model). Following Krautmann (2000), only the marginal revenue product of positional players (i.e., players other than pitchers) will be calculated. Krautmann points out that while the objective of positional players is primarily the production of runs, the marginal product of pitchers varies depending on whether the pitcher is a starter, a middle reliever, or a closer.

The varying objectives for these types of pitchers is difficult to distinguish in a systematic way in an analysis of ten years of pitcher data.

Following Scully, individual batter MRP is calculated based on the individual's contribution to the team's performance. Equation 2.12 provides the team's marginal revenue product for each performance increment. For example, an additional extra base translates into $X dollars in additional revenue. A player's MRP is thus calculated as the product of $X and the total number of bases that player generated. This is summarized below in equation 2.14, the MRP calculation for individual batters.

$$(2.14)\quad MRP_{i,t} = (\alpha_2 + \frac{\alpha_3}{1.07}) * (\beta_1 + \beta_3 * newstad_{j,t}) * totbase_{i,t}$$

This MRP calculation is then placed into the individual salary models of equation 2.6 (classic salary model) and equation 2.7 (alternative salary model) to yield equations 2.15 and equation 2.16, respectively.

$$\text{(Eq 2.15)}\quad \begin{array}{l} salary_{i,t} = \alpha_0 + \alpha_1(\exp_{i,t-1}) + \alpha_2(\exp^2{}_{i,t-1}) + \alpha_3(MRP_{i,t-1}) + \\ \alpha_4(newstad_{i,t-1}) + \mu_{it} + \varepsilon_i \end{array}, \text{ where:}$$

$$\text{(Eq. 2.16)}\quad \begin{array}{l} salary_{i,t} = \alpha_0 + \alpha_1(MRP_{i,t-1}) + \alpha_2(free_{i,t-1}) + \alpha_3(rook_{i,t}) + \alpha_4(star_{i,t-1}) + \\ \alpha_5(newstad_{i,t-1}) + \mu_{it} + \varepsilon_i \end{array},$$

where all variables are defined as before with the simple replacement of individual batter MRP for the player performance variable:

Once MRP is controlled in each model, the coefficient on the new stadium variable in each then becomes a measure of the rent-sharing effect on individual player salary. Note that even though individual pitcher MRP is not calculated, it is still possible to make an assessment of whether rent-sharing is present from the team level. For example, consider that equation 2.13 reveals that there is a negligible impact of a new stadium on team pitching performance, and thus there is a negligible impact of the stadium on the team MRP with respect to pitching. Now consider the individual salary model for pitchers in equation 2.6 and 2.7. If the coefficient on the new stadium dummy variable in these equations is positive, this information combined with the fact that new stadiums do not alter the marginal productivity of pitchers at the team level can be interpreted as evidence for rent-sharing.

If the analysis indicates that players benefit from rent sharing, one question that follows is the forcing mechanism. For instance, is the magnitude of rent sharing a function of union strength? Both Rose (1987) and Black and Strahan (2000) find for greater rent sharing for union members, although Black and Strahan found that non-union members benefited, whereas Rose did not. Unfortunately, for the purposes of this paper, baseball is highly unionized. Only a handful of players do no belong to the union so it will be difficult, if not impossible, to measure the relative strength of the union in capturing rents. An alternative is to consider whether star players have an advantage over non-stars in obtaining rents. I do this by modifying equations 2.6 (classic salary model) and 2.7 (alternative salary model) to include an interaction term of star and new stadium.

$$\begin{aligned} salary_{i,t} &= \alpha_0 + \alpha_1(\exp_{i,t-1}) + \alpha_2(\exp^2{}_{i,t-1}) + \alpha_3(perf_{i,t-1}) + \alpha_4(newstad_{i,t-1}) \\ &+ \alpha_5(star_{i,t-1}) + \alpha_6(star_{i,t-1} * newstad_{i,t-1}) + \mu_{it} + \varepsilon_i \end{aligned} \tag{2.16}$$

$$\begin{aligned} salary_{i,t} &= \alpha_0 + \alpha_1(perf_{i,t-1}) + \alpha_2(free_{i,t-1}) + \alpha_3(rook_{i,t-1}) + \alpha_4(star_{i,t-1}) + \\ &\alpha_5(newstad_{i,t-1}) + (star_{i,t-1} * newstad_{i,t-1}) + \mu_{it} + \varepsilon_i \end{aligned} \tag{2.17}$$
,

Equation 2.16 follows equation 2.6 with the addition of a dummy variable for star players and an interaction term between star players and new stadiums. Equation 2.17 follows equation 2.7 (alternative model) with the addition of an interaction term between star players and new stadiums. If the coefficient on the interaction term is positive, it points to the possibility that rent sharing, if it exists, is a function of some relative strength of one person in the market over another, in this case star power.

2-4: Data

2-4.1: Data Sources

With the models specified, a brief description of available data sources is provided. A reference table identifying sources is provided in Appendix 2.1. The sources are summarized here.

All player and team performance statistics were obtained from Sean Lahman's baseball database. This comprehensive database identifies individual performance statistics for players dating from 1871 to present and denotes Most Valuable Player awards and all-star team membership. The database also identifies team information such as annual attendance, win-loss records, and place finished.

The performance information from the Lahman database must be matched with financial information such as player salaries, team revenue figures, and stadium information.[31] Player salaries are available consistently from 1985 to present and sporadically prior to 1985. This salary information is posted on Professor Rod Fort's website at Washington State University. Fort identifies the primary source of this information, typically, the data is published annually in either the *USA Today* newspaper or *The Sporting News* magazine. Team revenue data are available from two sources. From 1990 to 1996, *Financial World Magazine* published detailed revenue information to include both total figures as well as a breakdown of revenue source to include gate receipts, broadcasting rights revenue, other stadium revenue, and other miscellaneous income. This information was summarized on a spreadsheet and provided by Professor Daniel Rascher. Team revenue information is also provided in Levin et. al's blue ribbon report done for the Commissioner of Baseball and reported to Congress. This information is available for 1995 to 1999. For the overlap years (1995 and 1996) between the *Financial World* data and the blue ribbon reports, the total revenue figures track closely often within one percent and typically by no more than five percent. For the overlap years, the average of the two revenue figures is used. Information about stadium construction is obtained from the *Sports Facility Reports* published periodically by the Marquette University Law School.

Considering the availability of the data, data analysis in this chapter focuses on the 1990s since revenue information, salary information, and performance data are relatively complete for this period. Intuitively, this period is appealing since it best represents the introduction of a new wave of stadium construction starting with the arrival of new stadiums in places such as Baltimore, Cleveland, and Chicago in the early 1990s.

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[31] This matching process was done with the aid of Microsoft® Access.

2-4.2: Descriptive summary

Table 2-1 identifies new stadiums built since 1990. For a point of reference, in 1990, there were twenty-six major league teams. The league expanded to twenty-eight in 1993 and to thirty in 1997. Between 1989 and 2001, fifteen new stadiums were built, representing new stadiums for half of the league. Of these fifteen stadiums, only two (Atlanta and San Francisco) have been completely built with private funds.[32] Of the remaining fifteen teams that have not received a new stadium in this period, two (Florida and Minnesota) had stadiums built in the 1980s. Interestingly, each is seeking public funds for new stadiums. Only five have no current plans for a new stadium within the next decade.[33] The analysis in this paper will focus on the effect of new stadiums (relative to the league) in Chicago, Baltimore, Cleveland, Colorado, and Atlanta since stadiums built for 1999-2001 are too recent for the dataset. Data of players from the two teams in Canada (Montreal and Toronto) are dropped because the financial information is incomplete. Data of players from the Tampa Bay and Arizona franchises are dropped because they were expansion franchises in 1997, too recent for inclusion.

The data in Table 2-2 reflect summary-level detail of individual salaries, MRP, and performance statistics for batters. The data in Table 2-3 reflect summary-level data of individual salaries and performance statistics for pitchers. The data are present by year for the period 1990 to 1999.

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[32] Even this is misleading; local communities often contribute infrastructure costs such as road improvements to support the stadium.

[33] These five teams are Anaheim (which received a major renovation in 1998), Chicago Cubs, Kansas City Royals, Los Angeles Dodgers, and Oakland Athletics.

2-5: Results

The first step in this process involves measuring the gross returns of new stadiums to batters and pitchers, as specified in equations 2.6 (classic salary model) and equation 2.7 (alternative salary model). Tables 2-4 and 2-5 reflect the results of the two proposed salary models, for batters and pitchers respectively. The "classic model" summarized in column (1) without fixed effects and in column (2) with fixed effects of each table, follows closely to Scully's (1974) model and subsequent updates to Scully. The "alternative model" summarized in column (3) without fixed effects and in column (4) with fixed effects is similar to the classic model but uses the non-linear experience classifications (i.e., rookie and free-agent status) instead of individual specific measures of experience.[34]

The results of the salary model for batters are provided in Table 2-4. The classic model of columns (1) and (2) explains about three-quarters of the variation in salary in either the model with or without the fixed effect specified. In either case, the experience variable and its square are signed as hypothesized and statistically significant, representing the idea that salary increases with experience at a decreasing rate. The performance variable also is significant in either case. The coefficient on performance means that each additional base generated at t-1 translates into additional salary of over three-tenths of one percent without fixed effects and over two-tenths of one percent with fixed effects. The stadium variable shows that there are positive returns to a new stadium

[34] Each of the salary models for batters includes total measure of performance as opposed to a mean measure. In 1994 and 1995, a certain number of games were canceled because of labor problems and this distorts the total figures for these two years. I therefore projected a full season worth of statistics using simple straight line projections.

of about 4% in the classic model without fixed effects shown in column (1), though the stadium effect on salary is negligible with the fixed-effect approach shown in column (2).

The results from the alternative salary model for batters are in Table 2-6, column (3) and (4). Overall, this model does not perform as well as the classic model as it only explains about 65% of the variation in player salary. Nevertheless, each independent variable is signed as hypothesized and is statistically significant. As expected, rookies who have little market freedom take a big hit in salary whereas free agent eligible players and all-stars benefit from their power in the labor market. In the fixed effects model, the estimate for the impact of a new stadium on salary is over 13% and this is statistically significant at better than five percent. In the model without fixed effects, the estimated impact of new stadiums is negligible.

The empirical data for batters therefore does not lend conclusive evidence that batters benefit from new stadiums. Of the four models summarized in Table 2-4, a case could be made that the classic model without fixed effects (column 1 of Table 3-4) and the alternative model with fixed effects (column 4 of Table 3-5) are the most appropriate. The rationale is that in the classic model, individual experience is controlled for at a detailed individual level (i.e., by the use of individual cumulative at bats and its square) and therefore the risk of omitted variable bias is relatively low. In this case, the more efficient random effects model is more appropriate. The relative risk of an omitted variable bias increases in the alternative model because individual experience variables are replaced with general labor experience categorization variables and thus the fixed effects version of the model may be more appropriate. In these two instances of column

(1) and column (4), the gross impact of new stadiums on salary is marginally significant in the case of the classic model or significant in the case of the alternative model. In any event, the strength for the finding of a gross effect of stadiums in Table 2-4 on batter salary is marginal at best.

The ability to test a distinct data set of individual pitcher salaries provides an opportunity to either strengthen or cast doubt on these findings for batters. The results in Table 2-5 reflect the findings for pitchers, again using the classic and an alternative salary model and also with and without a fixed effect specification. The classic model in columns (1) (with no fixed effects) and (2) (with fixed effects) explain 65% of the variation in pitcher salary. The performance and experience variables are both signed as hypothesized and statistically significant. For example, a one-unit increase in earned run average decreases salary by over one and a half percent. Salary increases with experience but at a decreasing rate. The stadium variable suggests that pitchers benefit from a new stadium by 13% in the fixed-effects specification to about 22% in the specification without fixed effects.

The alternative salary model for pitchers summarized in columns (3) and (4) of Table 2-7 explains over 40 percent of the variation in player salary. Each of the performance and experience variables is signed as hypothesized and is significant. The effect of new stadiums on salary ranges from 13% to 30% and this effect is significant in all four of the models.

As is the case with the batters, a case can be made that the classic model without a fixed effects specification in column (1) and the alternative with fixed effects in column

(4) are the most appropriate models of the four specifications for pitcher salary. Focusing on these two models, the estimate of a new stadium on a pitcher's salary is in the range of 22% to 30% and these findings are significant at the one percent level in both cases. Note that these estimates are higher for pitchers as compared to batters (4% to 13%) and suggest that pitchers have an advantage in obtaining benefits from new stadium.

The next phase of this section seeks to identify the source of these returns to batters and pitchers.[35] Do the salaries change because a player's marginal revenue product has changed or because rent sharing is present? Two ways of determining this are discussed in the methodology section. The first method described in equation 2.8 and equation 2.9 simply involves adding an interaction term for a new stadium and performance in the salary model. The idea is that the coefficient on this variable captures the MRP impacts of the new stadium on player salary (assuming that salary approximates MRP) while the new stadium dummy variable then picks up the rent-sharing effects. The second method involves computing team and individual MRPs and evaluating those within the salary framework. In the case of batters, individual MRP is entered into the individual salary model as an independent variable. Each method is addressed in turn.

The results in Table 2-6 reflect the results for batters of the salary model described in equation 2.8 (classic salary model with interaction term) and equation 2.9 (alternative salary model with interaction term). These models simply include the addition of an interaction variable for performance within a new stadium. Assuming that player salary approximates MRP, controlling for relevant performance and experience variables, the coefficient on this interaction variable captures the MRP effect of the stadium on player salary. The coefficient on the new stadium dummy variable is then assumed to capture rent-sharing effects of the stadium on player salary. In all four models summarized in Table 2-6, the coefficient on the interaction term is insignificant, though the alternative model without a fixed effect suggests that performance in a new facility actually lowers salary but this effect is not statistically significant. The coefficient on the new stadium variable dummy variable is also insignificant in all four cases suggesting that batters do not benefit from the rents of new stadiums either.

The empirical data for pitchers depicted in Table 2-7, however, paint a different picture. In Table 2-7, it is shown that the coefficient on the interaction term is insignificant in all four cases suggesting a negligible impact of a new stadium on pitcher salary by way of the stadiums impact on pitcher performance. In three of the four cases, however, the coefficient on the new stadium dummy variable is positive and significant, ranging from 27.4% to 35.5% in the three significant cases, suggesting that pitchers are relatively successful in the sharing of rents, as compared to the case of batters.

The second manner in which the source of new stadium benefits on player salary are distinguished is through the calculation of team and individual marginal revenue products and then controlling for MRP in the salary equation models. The calculation of MRP is detailed in equations 2.10 through equation 2.14 of the methodology section. The revenue model in equation 2.10 suggests that team revenue is a function of the regional population, team on-field performance as measured by the team's winning

[35] Note that while the impact of new stadiums on batters is marginal at best, I will nonetheless attempt to

percentage for the current and previous year, a dummy variable for each season, and a dummy variable that indicates whether a team plays in a new stadium.

The results from this model are provided in Table 2-8, column (1) through column (4). In column (1) and column (2), the results are from using total revenue as the dependent variable, with and without a fixed effect specification respectively. In column (3) and column (4), the results are from using the log of total revenue as the dependent variable, with and without a fixed effect specification respectively. A Hausman test fails to reject the null hypothesis that there is no systematic difference in the fixed and random effect specifications. Considering this, the more efficient random effect estimates presented in column (2) and column (4) are preferable to the less efficient fixed effect estimates and will be discussed below.

Overall, the models summarized in columns (2) and (4) of Table 2-8 explain about 70 percent of the variation in team revenue. Population, winning percentage, and lagged winning percentage are signed as anticipated and are significant at better than the one percent level. A one percent increase in the team's current winning percentage, for example, translates into about $525,000 in additional revenue as shown in column (2) or about nine-tenths of one percent increase in additional revenue as shown in column (4). Lagged winning percentage also shows to be an important indicator of team revenue and is in fact greater in magnitude than current winning percentage. This likely owes in part to the fact that a large percentage of a stadium's seats are sold prior to season start and in part reflects support for the team generated from the previous year's success. The strike

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identify the sources of movement in batter salary.

years of 1994 and 1995 had a significant negative impact on team revenue especially in 1995 in which revenue dropped 12,100,000 or by 27%. Finally, new stadiums provide a windfall to teams increasing revenue by over 20% or $13,000,000 on average.

Note that I have not explicitly dealt with stadium capacity in the team revenue function. For example, consider the case that new stadiums were built with increased capacity relative to the old stadiums. This should be taken into account because the increased capacity would enable more fans to attend the games and this would increase player MRP in the new stadium. It should be noted, however, that of the new stadiums built throughout the 1990s that were included in this study, only in one of the six cases (in Arlington, Texas) did capacity increase. In Chicago, Baltimore, Cleveland, Colorado, and Atlanta, capacity decreased in the new stadium so it would not appear to be the case that old stadium capacity acted as a constraint on player MRP.[36]

As mentioned, the return represented by the new stadium dummy variable in Table 2-8 represents the marginal benefit largely independent of the team's on-field performance. To see how a new stadium impacts revenue through its effect on on-field performance, it is necessary to see how the stadium influences a team's winning percentage. First, as shown in Table 2-8, a team's winning percentage significantly influences team revenue. Next, then, the team's winning percentage is modeled as a

[36] In any event, I re-ran the team revenue equation with stadium capacity added as an explanatory variable. The coefficient on capacity is negative and significant. The coefficient on winning percentage changed from 52,500,000 to 47,900,000. The coefficient on lagged winning percentage changed from 58,400,000 to 54,700,000. The coefficient on the new stadium dummy variable changed from $13,000,000 to $10,700,000. These changes in the coefficients alter the calculation of individual batter MRP but do not appreciably alter the core findings.

function of team offensive and pitching performance and specifically includes interaction variables to measure how a new stadium influences this performance.

The results in Table 2-9 reflect the regression of team winning percentage on the specified independent variables. Overall, the model explains over three-quarters of the variation in the team's winning percentage. As expected, the number of extra bases a team achieves has a significant impact on a team's winning percentage. The coefficient on total bases (0.00021) is difficult to conceptualize. As a way to conceive it, consider the fact that one game in a 162 game season represents six-tenths of one percent (0.006) of the season. In order to earn one additional win, a team would have to achieve 28 additional extra bases (=0.006/0.00021) throughout the season in order to expect to achieve this additional win. Also, as expected, the team's earned run average also has a significant impact on the team's winning percentage. Since a higher earned average signifies worse performance, the coefficient on this variable is negative. For every one run increase in the team's earned run average, the team can expect a ten percent decrease in its winning percentage which translates into about 16 fewer wins during a 162 game season.

An interesting result in Table 2-9 is that it takes more extra bases in the new stadium to produce a one unit increase in winning percentage, as compared to the old stadium. For a given level of performance, therefore, a batter's marginal revenue product is higher in the old stadium. The situation is similar for pitchers. In a stadium that is not new, a one run decrease in earned run average improves winning percentage by almost ten percent. In a new stadium, a one run decrease in earned average improves winning percentage by only nine percent. One possible explanation for this is that unlike sports such as basketball and football, the physical playing field in baseball is not uniform from stadium to stadium. In some stadiums, for example, the fences may be further out and thus it is harder to hit homeruns whereas in others, the fences are short and homeruns are more frequent. It is not uncommon to hear stories of team management signing players to fit a particular stadium since some types of player may be better suited to certain conditions. With a new ballpark, there may be a period of adjustment until the team can determine and then assemble the best fit of player to the new stadium. Hence, there is the potential of a negative performance effect during the initial years of the park. Note however, that neither coefficient on the interaction terms are significantly different from zero and that it may be the case that a new stadium has no impact on team performance, ceteris paribus.

In any event, this finding suggests a new stadium potentially has a negative impact on a team's MRP for both batters and pitchers because the new stadium negatively impacts the marginal product portion of the MRP calculation. In the individual salary models for pitchers (Table 2-5), the coefficient on the new stadium variable suggests that pitchers nonetheless benefit in terms of higher salary from new stadiums. Although I don't plan to estimate individual MRP for pitchers for reasons previously mentioned, the finding at the team level that new stadiums may negatively impact pitching performance suggests that individual pitchers benefit from rent-sharing. In other words, pitchers receive increased salaries when playing in new stadiums despite the fact that a new stadium tends to decrease their marginal value. This also supports the findings from the first approach used to disentangle the effects of a new stadium on pitcher salary summarized in Table 2-7, in which a new stadium-performance interaction variable is added to the individual salary models to separate MRP and rent-sharing effects.

Using the information from the team level, it is possible to estimate individual batter MRP, using equation 2.14, which is re-stated here.

$$(2.14)\quad MRP_{i,t} = (\alpha_2 + \frac{\alpha_3}{1.07}) * (\beta_1 + \beta_3 * newstad_{j,t}) * totbase_{i,t}$$

In equation 2.14, α_2 and α_3 are the coefficients from winning percentage and lagged winning percentage in the team revenue function summarized in column (2) of Table 2-8, specifically 52,500,000 and 58,400,000 respectively. β_1 and β_3 are the coefficients from the total base performance variable and the total base-news stadium interaction variable from Table 2-9, specifically 0.00021 and -.000015, respectively. If a batter does not play on a team with a new stadium, his MRP would be calculated as follows:

$$(2.14a)\quad MRP_{i,t} = (52{,}500{,}000 + \frac{58{,}400{,}000}{1.07}) * (0.00021) * totbase_{i,t} = 22{,}408 * totbase_{i,t}$$

If a batter does play on a team with a new stadium, his MRP would be calculated as follows:

(2.14b)

$$MRP_{i,t} = (52{,}500{,}000 + \frac{58{,}400{,}000}{1.07}) * (0.000210 - 0.000015) * totbase_{i,t} = 20{,}759 * totbase_{i,t}$$

MRP for each batter is thus calculated for each batter by multiplying the batter's total bases for a season by one of the two factors in equation 2.14a or equation 2.14b depending on whether the player played in a new stadium. This MRP calculation then can be inserted into the individual salary equations replacing the performance measure, as shown in equation 2.15 and equation 2.16. The results from this process are provided in Table 2.10. Table 2-10 reveals that controlling for individual batter MRP, batters benefit from playing in a new stadium in three of the four specified models, from 16% to 27% in the significant cases. This provides some support for the hypothesis that rent sharing is present for batters. This, however, conflicts with the findings from the first approach used to separate MRP and rent sharing effects on player salary in which it was suggested that batters do not benefit from rent-sharing, after adding an interaction variable to the individual salary model for batter performance in the new stadium. It is possible that the calculation of individual MRP in this second approach is at such a crude level so as to generate spurious results.

The empirical evidence summarized to this point suggests that pitchers are successful in obtaining some amount of rents. The case for batters is much less clear although it is possible that batters are also successful to some degree in obtaining rents. This suggests that there is some characteristic of pitchers (as compared to batters) that enable them to obtain higher rents. One possibility is that teams with new stadiums are more active in signing free agent players using the additional revenue from the stadiums. The relative scarcity of quality pitchers in the free agent market may prompt the bidding teams to engage in more speculative bidding on pitchers as opposed to batters.

Another approach is to consider whether a subset of players is more successful in obtaining rents. One possible subset is that of all-star players. This is accomplished by adjusting the salary equation models for both batters and pitchers to include an interaction term between a player's all-star status and whether the player played on a team with a new stadium. The results from this exercise are provided in Table 2-8 (batters) and Table 2-9 (pitchers). The findings here suggest that star players do not have a relative advantage. The coefficient on the interaction term for star status and new stadium is not significant in any of the cases for batters. The sign on this coefficient is positive for each possible model for pitchers, with two models that reveal this effect as significant. The relative advantage for obtaining rents therefore appears to extend to pitchers as compared to batters , particularly all-star pitchers. All-star batters, as a subset of overall batters, however do not appear to have a relative advantage.

As a final point to this chapter, it is helpful to provide a sense of the order of magnitude of the rents shared between owners and players. It is possible to provide a rough estimate of this by examining team-level revenue, payroll, and on-field success. In the team revenue equation, the coefficient on the new stadium dummy variable is \$13,000,000 (reference Column (2) of Table 2-8), suggesting that a new stadium translates into additional revenue of about \$13 million per year for the team. Now consider the regression of team total payroll on local market size and a new stadium dummy variable. The results are provided in Table 2-13 and suggest that a new stadium implies that a team will increase payroll by \$6,348,142 or just under half of the total annual rent received. The increase in payroll in turn has an effect on winning percentage which can be seen by regressing team winning percentage on payroll, displayed in Table 2-14. The coefficient on payroll (3.56e-09) means that a team would have to spend \$2,808,988 (i.e., 0.01/3.56e-09) in payroll to get a one percent increase in winning percentage. So, a team that spends \$6,348,142 in additional payroll from new stadium rents can expect to increase winning percentage by 2.25%. An increase in winning percentage of 2.25% has a marginal impact on team revenue of \$2,676,980, obtained by multiplying 2.25% by the sum of the coefficients on winning percentage and lagged winning percentage[37] from the team revenue function in Table 2-8.

To recap, this sequence of calculations suggests that owners spend on average \$6,348,142 in additional payroll from new stadium rents and gain only \$2,676,980 in marginal revenue from the winning that this additional payroll generates. At least a part of the difference between these numbers (\$3,671,162) can be interpreted as the rent received by players on average from each team with a new stadium for each year. A portion, however, can also be attributed to the difference of ex ante expectation for the marginal revenue to be gained by the additional payroll and the ex-post realization.

2-6: Conclusion

The preceding analysis addresses the two questions posed at the start of this chapter. First, players do in fact benefit at a gross level from playing in new stadiums by way of increased salary. This is clearly the case for pitchers, although it is less certain for batters. Second, rent sharing effects are the source of this benefit, as opposed to a change in the player's marginal revenue product.

[37] The lagged value is discounted by 7% following Krautmann (2000) and others.

These findings contribute to two disparate strands of research. First, the economic study of athletic venues has been broadened to include the economic impact on professional athletes. The players have a vested interest in the construction of new venues, particularly in instances where the stadium is publicly funded. Their interest in public over private financing is driven by two factors. First, as discussed in section 2-4, the probability that a new stadium will be built is greater if it is publicly-funded. Second, the returns to players from new stadiums result from rent sharing as opposed to the stadium's effect on marginal productivity. The owners would presumably have a lower pool of money to share if forced to use private funds to build venues as opposed to receiving public subsidies. In turn, the introduction of the players as having a vested interest in public stadiums changes the dynamics of the stadium approval to include not only the team owners, but also the players. The extent of the players' actual involvement in seeking new publicly-financed stadiums is a separate question to be pursued in follow-on research.

Secondly, the research on the testing of rent sharing hypotheses has been broadened to include a field in which more precise measurements of individual performance and value are used. This provides an incisive way to separate wage changes that result from rent sharing as opposed to other factors, such as compensating wage differentials. In addition to being able to control for individual performance at such a detailed level, this paper is unique from both Rose (1987) and Black and Strahan (2001) in that they used deregulation as the event to find for rent sharing. That is, they found that payments to workers went down after deregulation, suggesting the presence of rent sharing prior to deregulation. This paper uses the construction of new stadiums as an act of monopolization and found that payments increased after the act, therefore suggesting rent sharing as a manifestation of monopoly power. In any event, the finding of rent sharing here supports the earlier findings and helps demonstrate that the previous findings are robust to the granularity of the data used and the approach employed.

The approach to the methods used here to find for rent sharing effects are naturally open to criticism. One criticism involves the definition of what baseball players produce and subsequently how marginal revenue product (MRP) is defined. In this paper, the production objective of players is defined to mean the production of winning baseball games. The impact of stadiums on a player's MRP was thus targeted to only how a new stadium impacts the players' ability to win games, which in turn has a marginal impact on stadium attendance and the ability to receive more lucrative media contracts. The impact of new stadiums on team revenue outside of this narrow scope was declared a rent up for grabs so to speak. If, however, the production of baseball players were more broadly defined to include, for instance, the players' part in creating an attractive baseball-going experience for fans, then that which was declared a rent to players in this chapter would probably not be a rent in this broader definition. In this broader definition, players and stadiums are complementary inputs to the game-going experience and the stadiums would have a larger impact on the player's MRP than as defined here.

A rebuttal to this criticism would involve an evaluation of the salary negotiation process. As an outside but fairly regular observer of the process, I would suggest that anecdotal evidence indicates that salary negotiations between a team and any given player are predominantly tied to on-field performance. This then supports the narrow definition of the player production function in this paper. The overall economic health of baseball and how new stadiums have improved this health, is typically a matter of discussion between owners and players during collective bargaining and therein likely lies a critical link to the players' success in benefiting from these rents. A deeper evaluation of how success in collective bargaining in turn translates into increases in individual salary is an area for future research.

From a more general theoretical standpoint, findings of rent sharing have interesting implications for economic theory. Traditional theory assumes that worker wages are not a function of the employers' market power. In other words, the monopoly owner retains all monopoly rents. The fact that rents are shown to be shared may enable theoretical development and re-examination to include: 1) a re-examination of rent-seeking coalitions, to include not only the monopoly owners but also the factors of production; 2) a re-examination of rent-seeking costs, to include not only the efforts of the monopoly owners to obtain rents but also to those who seek entrance into the particular field; 3) an examination of the forcing mechanisms of rent sharing such as the power of unions to attract relatively greater rents; and, 4) the development of an alternative model to efficiency wages and labor market signaling, among others, to explain differential wages paid to an otherwise competitive labor market.

The field of sports provides a rich area for developing and testing economic theory. Both individual (player) and corporate (team) data are readily available and this opens doors for empirical testing, particularly at the individual unit of observation. This has not been a luxury in other studies. This paper taps into the available data and offers unique ways to expand both the literature of sports economics and to strengthen efforts to test for rent sharing effects.

Table 2-1: New Stadium Construction in the 1990s

Team	Stadium	Year Open	% Public[38]
Tampa Bay Devil Rays	Tropicana Field	1990	100%
Chicago White Sox	New Comiskey Park	1991	100%
Baltimore Orioles	Camden Yards	1992	96%
Cleveland Indians	Jacobs Field	1994	88%
Texas Rangers	Ballpark at Arlington	1994	80%
Colorado Rockies	Coors Field	1995	75%
Atlanta Braves	Turner Field	1997	0%
Arizona Diamondbacks	Bank One Ballpark	1998	76%
Seattle Mariners	Safeco Field	1999	76%
Detroit	Comerica Park	2000	50%
Houston	Enron Field	2000	67%
San Francisco	Pacific Bell Park	2000	0%
Milwaukee	Miller Park	2001	64%
Pittsburgh	PNC Park	2001	71%

[38] The percentage assessment comes from the *Sports Facility Reports*. It is subjective in the sense that it may not include infrastructure costs such as improved road access to a stadium, typically paid by the community. In this light, public percentages may be understated.

Table 2-2: Summary Detail Of Variables, Batters, 1990-1999 ($1990 Constant)

Year	Variable	Minimum	Average	Maximum	# of Obs
1990	Salary	$100,000	$696,110	$3,200,000	387
	MRP	$0	$2,964,111	$7,982,118	387
	Total Bases	0	165	445	387
1991	Salary	$95,961	$856,834	$3,646,549	391
	MRP	$0	$2,843,642	$8,269,116	391
	Total Bases	0	159	461	391
1992	Salary	$101,542	$1,013,978	$5,682,609	421
	MRP	$0	$2,690,303	$8,269,116	421
	Total Bases	0	150	461	421
1993	Salary	$90,449	$1,014,261	$5,607,889	449
	MRP	$0	$2,858,829	$9,614,416	449
	Total Bases	0	159	536	449
1994*	Salary	$96,129	$1,082,778	$5,556,073	420
	MRP	$0	$2,388,016	$7,426,061	420
	Total Bases	0	133	414	420
1995*	Salary	$93,480	$1,057,735	$7,922,187	481
	MRP	$0	$2,614,565	$8,430,552	481
	Total Bases	0	145	470	481
1996	Salary	$90,799	$949,224	$7,694,973	545
	MRP	$0	$2,756,690	$8,968,672	545
	Total Bases	0	153	500	545
1997	Salary	$122,149	$1,145,831	$8,143,302	524
	MRP	$0	$2,707,854	$9,237,732	524
	Total Bases	0	151	515	524
1998**	Salary	$136,312	$1,165,616	$8,018,405	489
	MRP	$0	$3,018,752	$10,040,000	489
	Total Bases	0	168	579	489
1999	Salary	$156,903	$1,393,097	$9,374,778	466
	MRP	$0	$3,250,662	$9,309,481	466
	Total Bases	0	181	519	466

*A player's strike forced the cancellation of 50 games for each team at the end of 1994 and 18 for each team at the beginning of 1995. The salaries do not reflect the fewer amount of games; however, the fewer games are reflected in the performance data.
**The salary data for 1998 included pro-rated salaries of rookies called to the major leagues for brief stints of time. Since no other years contained this type of data, these observations were dropped.

Table 2-3: Summary Detail of Variables, Pitchers, 1990-1999 ($1990 Constant)

Year	Variable	Minimum	Average	Maximum	# of Obs
1990	Salary	$100,000	$604,349	$2,650,000	317
	Innings	0	103	267	317
	Runs Allowed	0	43	125	317
1991	Salary	$95,961	$915,617	$3,478,616	302
	Innings	0	101	271	302
	Runs Allowed	0	43	130	302
1992	Salary	$101,542	$1,037,151	$4,580,245	330
	Innings	1	98	268	330
	Runs Allowed	0	40	118	330
1993	Salary	$90,450	$986,522	$5,351,615	367
	Innings	0	91	267	367
	Runs Allowed	0	42	127	367
1994*	Salary	$96,129	$971,965	$4,674,157	337
	Innings	2	72	202	337
	Runs Allowed	0	36	107	337
1995*	Salary	$93,480	$825,250	$6,860,893	426
	Innings	0	75	222	426
	Runs Allowed	0	36	114	426
1996	Salary	$90,799	$680,308	$5,560,373	473
	Innings	1	82	266	473
	Runs Allowed	0	41	134	473
1997	Salary	$101,791	$832,811	$6,840,374	471
	Innings	0	80	264	471
	Runs Allowed	0	38	135	471
1998**	Salary	$136,313	$906,524	$7,697,669	414
	Innings	0	86	251	414
	Runs Allowed	0	32	130	414
1999	Salary	$156,903	$1,321,831	$8,629,651	393
	Innings	0	89	272	393
	Runs Allowed	0	46	140	393

*A player's strike forced the cancellation of 50 games for each team at the end of 1994 and 18 for each team at the beginning of 1995. The salaries do not reflect the fewer amount of games; however, the fewer games are reflected in the performance data.
**The salary data for 1998 included pro-rated salaries of rookies called to the major leagues for brief stints of time. Since no other years contained this type of data, these observations were dropped.

Table 2-4: Individual Salary Model (Batters), 1990-1999

	Dependent Variable is Log Salary			
	Classic Model		Alternative Model	
	(1)	(2)	(3)	(4)
Performance Variable	0.0035***	0.0026***	0.0057***	0.0041***
(Total Bases Lagged)	(31.89)	(17.25)	(45.19)	(22.34)
Experience	0.0008***	0.0011***		
(Cum. At-Bats to t-1)	(46.67)	(38.19)		
Experience Squared	-7.54e-08***	-9.51e-08***		
	(30.96)	(25.19)		
New Stadium	0.039	-0.053	-0.011	0.136**
(=1 if new stad. at t-1)	(0.94)	(0.95)	(0.22)	(2.04)
Rookie			-0.434***	-0.582***
(=1 if <2 years exp.)			(9.36)	(10.17)
Free-Agent Eligible			0.913***	0.764***
(=1 if >5 yrs. exp. at t-1)			(32.87)	(19.47)
Star			0.311***	0.200***
(=1 if All-Star at t-1)			(6.31)	(3.37)
Player Fixed Effect?	No	Yes	No	Yes
No. of Observations	3331	3331	3331	3331
R^2	0.75	0.74	0.64	0.64

1. Unit of observation is the log of individual player salary
2. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2-5: Individual Salary Model (Pitchers), 1990-1999

	Dependent Variable is Log Salary			
	Classic Model		Alternative Model	
	(1)	(2)	(3)	(4)
Performance Variable (Earned Run Avg.)	-0.0161*** (4.82)	-0.0164*** (4.01)	-0.0230*** (5.44)	-0.0153*** (2.98)
Experience (Innings Pitched to t-1)	0.0036*** (49.92)	0.0042*** (39.09)		
Experience Squared	-1.06e-06*** (32.60)	-1.20e-06*** (27.31)		
New Stadium (=1 if new stad. at t-1)	0.218*** (4.06)	0.132* (1.91)	0.209*** (3.07)	0.300*** (3.53)
Rookie (=1 if <2 years exp.)			-0.867*** (15.92)	-0.895*** (13.89)
Free-Agent Eligible (=1 if >5 yrs. Exp. at t-1)			1.176*** (28.30)	0.8713*** (16.22)
Star (=1 if All-Star at t-1)			0.637*** (8.41)	0.2417*** (3.11)
Player Fixed Effect?	No	Yes	No	Yes
No. of Observations	2509	2509	2509	2509
R^2	0.65	0.65	0.44	0.42

1. Unit of observation is the log of individual player salary
2. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2-6: Individual Salary Model (Batters with New Stadium*Performance Interaction), 1990-1999

	Dependent Variable is Logged Salary			
	Classic Model		Alternative Model	
	(1)	(2)	(3)	(4)
Performance Variable (Total Bases Lagged)	0.0035*** (30.43)	0.0026*** (16.72)	0.0057*** (43.81)	0.0040*** (21.69)
Experience (Cum. At-Bats to t-1)	0.0008*** (46.64)	0.0010*** (38.19)		
Experience Squared	-7.54e-08*** (30.97)	-9.52e-08*** (25.19)		
New Stadium (=1 if new stad. at t-1)	0.016 (0.21)	-0.091 (0.87)	0.069 (0.77)	0.080 (0.63)
Stad.*Perf. Interaction (New Stad. at t-1*Perf. At t-1)	0.0001 (0.35)	0.0002 (0.43)	-0.0003 (1.05)	0.0003 (0.54)
Rookie (=1 if <2 years exp.)			-0.433*** (9.32)	-0.584*** (10.18)
Free-Agent Eligible (=1 if >5 yrs. exp. at t-1)			0.913*** (32.86)	0.761*** (19.53)
Star (=1 if All-Star at t-1)			0.313*** (6.35)	0.1983*** (3.35)
Player Fixed Effect?	No	Yes	No	Yes
No. of Observations	3331	3331	3331	3331
R^2 (overall)	0.75	0.74	0.64	0.64

1. Unit of observation is the log of individual player salary
2. As discussed in paper, the interaction term (stadium*performance) is included to measure MRP effects of a new stadium on player salary. The new stadium dummy variable then captures all other effects on salary, principally rent-sharing effects.
3. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2-7: Individual Salary Model (Pitchers with New Stadium*Performance Interaction), 1990-1999

	Dependent Variable is Logged Salary			
	Classic Model		Alternative Model	
	(1)	(2)	(3)	(4)
Performance Variable (Earned Run Avg.)	-0.0157*** (4.63)	-0.0168*** (4.00)	-0.0225*** (5.24)	-0.0147*** (2.83)
Experience (Innings Pitched to t-1)	0.0036*** (49.92)	0.0042*** (39.09)		
Experience Squared	-1.06e-06*** (32.60)	-1.20e-06*** (27.29)		
New Stadium (=1 if new stad. at t-1)	0.274*** (2.77)	0.100 (0.90)	0.279** (2.22)	0.355*** (2.57)
Stad.*Perf. Interaction (New Stad. at t-1*Perf. At t-1)	-0.013 (0.67)	0.0075 (0.36)	-0.0182 (0.71)	-.0130 (0.50)
Rookie (=1 if <2 years exp.)			-0.867*** (15.93)	-0.896*** (13.89)
Free-Agent Eligible (=1 if >5 yrs. Exp. at t-1)			1.18*** (28.29)	0.8716*** (16.22)
Star (=1 if All-Star at t-1)			0.635*** (8.38)	0.2406*** (3.09)
Player Fixed Effect?	No	Yes	No	Yes
No. of Observations	2509	2509	2509	2509
R^2	0.65	0.65	0.44	0.42

1. Unit of observation is the log of individual player salary
2. As discussed in paper, the interaction term (stadium*performance) is included to measure MRP effects of a new stadium on player salary. Once the interaction term is included, the new stadium dummy variable then captures all other effects on salary, principally rent-sharing effects.
3. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2-8: Team Revenue, 1990-1999

	Dep. Var: Team Revenue	Dep. Var: Team Revenue	Dep. Var: Log Rev	Dep. Var: Log Rev
	(1)	(2)	(3)	(4)
Population	0.44	1.73***	-3.72e-08	2.74e-08***
(Metropolitan Pop)	(0.11)	(5.44)	(0.59)	(5.20)
Winning %	51,700,000***	52,500,000***	0.90***	0.90***
(at time t)	(6.33)	(6.46)	(6.90)	(6.90)
Lagged Win %	58,500,000***	58,400,000***	0.97	0.96
(at time t-1)	(7.37)	(7.36)	(7.66)	(7.54)
Season 1991	2,639,702	2,525,692	0.05	0.05
	(1.27)	(1.21)	(1.63)	(1.50)
Season 1992	3,531,064*	3,299,030	0.07**	0.06*
	(1.66)	(1.57)	(2.14)	(1.91)
Season 1993	5,388,182**	5,388,182**	0.11***	0.10***
	(2.45)	(2.45)	(3.22)	(2.98)
Season 1994	-650,900	-954,201	-.002	-.016
	(0.29)	(0.46)	(0.05)	(0.47)
Season 1995	-11,700,000***	-12,100:***	-0.25***	-0.27***
	(5.13)	(5.81)	(6.94)	(8.09)
Season 1996	-144,287	-569,542	-0.001	-0.021
	(0.06)	(0.27)	(0.04)	(0.63)
Season 1997	6,791,165***	6,285,712***	0.12***	0.10***
	(2.73)	(2.73)	(3.04)	(2.92)
Season 1998	12,400,000***	11,800,000***	0.21***	0.18***
	(4.71)	(5.69)	(5.00)	(5.00)
Season 1999	20,400,000***	19,800,000***	0.33***	0.30***
	(7.29)	(9.55)	(7.39)	(9.02)
New Stadium	11,900,000***	13,000,000***	0.21***	0.22
(=1 if new at t)	(5.89)	(6.57)	(6.44)	(7.10)
Team Fixed effect?	Yes	No	Yes	No
No. of Obs.	252	252	252	252
R^2 (overall)	0.57	0.69	0.02	0.70

1. Unit of observation is team revenue in column (1) and the log of team revenue in column (2).
2. t-statistics in parentheses
   *=Significant at 10 percent level; *=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2-9: Team Winning Percentage, 1990-1999

	Dependent Variable: Team Winning %
	(1)
Total Bases (at time t)	0.00021*** (20.69)
Team Earned Run Average (at time t)	-0.0990*** (21.01)
Total Bases*New Stadium (at time t-1)	-0.000015 (0.92)
Earned Run Avg.*New Stadium	0.01066 (0.89)
Team Fixed effect?	Yes
No. of Observations	254
R^2 (overall)	0.77

1. Unit of observation is the winning percentage of each team in the major leagues from 1990 to 1999 excluding Toronto, Montreal, Colorado, and Arizona.
2. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2-10: Individual Salary Model Controlling for MRP (Batters), 1990-1999

	Dependent Variable is Log Salary			
	Classic Model		Alternative Model	
	(1)	(2)	(3)	(4)
MRP (Log of MRP, t-1)	0.225*** (19.54)	0.0019*** (13.69)	0.438*** (33.28)	0.259** (14.51)
Experience (Cum. At-Bats to t-1)	0.0009*** (48.43)	0.0011*** (43.17)		
Experience Squared	-8.43e-08*** (31.30)	-1.04e-07*** (27.84)		
New Stadium (=1 if new stad. at t-1)	0.162*** (3.65)	0.006 (0.10)	0.181*** (3.42)	0.274** (3.92)
Rookie (=1 if <2 years exp.)			-0.427*** (8.22)	-0.698*** (11.49)
Free-Agent Eligible (=1 if >5 yrs. exp. at t-1)			0.934*** (30.53)	0.825*** (20.10)
Star (=1 if All-Star at t-1)			0.863*** (17.16)	0.483*** (8.06)
Player Fixed Effect?	No	Yes	No	Yes
No. of Observations	3331	3331	3331	3331
R^2	0.71	0.71	0.58	0.64

1. Unit of observation is the log of individual player salary
2. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2-11: Individual Salary Model (Star Batters and New Stadiums), 1990-1999

	Dependent Variable is Logged Salary			
	Classic Model		Alternative Model	
	(1)	(2)	(3)	(4)
Performance Variable (Total Bases Lagged)	0.0032*** (27.28)	0.0025*** (16.22)	0.0057*** (45.19)	0.0041*** (22.34)
Experience (Cum. At-Bats to t-1)	0.0008*** (47.08)	0.0011*** (37.97)		
Experience Squared	-7.64e-08*** (31.51)	-9.46e-08*** (24.98)		
New Stadium (=1 if new stad. at t-1)	0.044 (0.98)	-0.004 (0.70)	0.105 (0.20)	0.127*** (2.60)
Rookie (=1 if <2 years exp.)			-0.434*** (9.35)	-0.583*** (10.17)
Free-Agent Eligible (=1 if >5 yrs. exp. at t-1)			0.913*** (32.85)	0.761*** (19.52)
Star (=1 if All-Star at t-1)	0.267*** (6.05)	0.0881* (1.67)	0.328*** (6.28)	0.193*** (3.08)
Star*New Stad. (Interaction for t-1)	-0.062 (0.40)	-0.0533 (0.43)	-0.123 (0.97)	0.046 (0.32)
Player Fixed Effect?	No	Yes	No	Yes
No. of Observations	3331	3331	3331	3331
R^2	0.75	0.74	0.65	0.64

1. Unit of observation is the log of individual player salary
2. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2-12: Individual Salary Model (Star Pitchers and New Stadiums), 1990-1999

	Dependent Variable is Logged Salary			
	Classic Model		Alternative Model	
	(1)	(2)	(3)	(4)
Performance Variable (Earned Run Avg.)	-0.015*** (4.54)	-0.016*** (3.84)	-0.0258*** (5.84)	-0.0153*** (2.98)
Experience (Innings Pitched to t-1)	0.0035*** (49.64)	0.0042*** (38.92)		
Experience Squared	-1.04e-06*** (32.55)	-1.20e-06*** (27.17)		
New Stadium (=1 if new stad. at t-1)	0.189*** (3.41)	0.0723 (1.12)	0.200*** (2.80)	0.283*** (3.17)
Rookie (=1 if <2 years exp.)			-0.867*** (15.92)	-0.895*** (13.88)
Free-Agent Eligible (=1 if >5 yrs. Exp. at t-1)			1.176*** (28.29)	0.871*** (16.21)
Star (=1 if All-Star at t-1)	0.421*** (6.60)	0.1089* (1.65)	0.624*** (7.80)	0.226*** (2.77)
Star*New Stad. (Interaction for t-1)	0.231 (1.27)	0.3882** (2.03)	0.200** (2.80)	0.145 (0.61)
Player Fixed Effect?	No	Yes	No	Yes
No. of Observations	2509	2509	2509	2509
R^2	0.65	0.65	0.44	0.42

1. Unit of observation is the log of individual player salary
2. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2-13: Team Payroll, 1990-1999

	Dep. Var: Team Payroll
Population (Metropolitan Pop)	18.74*** (4.33)
New Stadium (at time t)	6,348,142*** (6.33)
Yearly Dummy (Dummy for each Year)	Output suppressed
Team Fixed effect?	Yes
No. of Obs.	254
R^2 (overall)	0.11

1. Unit of observation is team revenue in column (1) and the log of team revenue in column (2).
2. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2-14: Team Winning Percentage, 1990-1999

	Dep. Var: Team Winning Percentage
Payroll	3.56e-09*** (8.04)
Yearly Dummy (Dummy for each Year)	Output suppressed
Team Fixed effect?	Yes
No. of Obs.	256
R^2 (overall)	0.29

1. Unit of observation is team revenue in column (1) and the log of team revenue in column (2).
2. t-statistics in parentheses

*=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Chapter 3: The Distributive Politics of Defense Spending

This chapter provides an empirical examination of the distributive politics of defense spending. Two subsets of military spending are considered: 1) annual prime contract awards from the Department of Defense to each state from 1963 to 2000; and 2) military construction earmarks approved by legislators from 1995 to 2000 in the annual appropriations process, but which were not part of the President's original budget request. The consideration of the first dataset extends previous studies that have attempted to explain the political distribution of contracts using data through as recently as 1989. A benefit of including the 1990s is that it allows insight into potential shifts in distributive forces as Congress grappled with the collapse of the Soviet Union and the drawdown of defense spending in the United States.

The use of the alternative earmark dataset enables a unique approach within the defense distributive politics literature by examining military pork at the congressional district level. In addition, this alternative dataset represents a subset of the defense budget (i.e., military construction) that represents an identifiable higher-payoff and more certain target for a representative seeking to win support in the home district.

In total, the consideration of these two distinct datasets within defense sheds light on the defense budget as heterogeneous, with some components of the budget more conducive to political distribution than others. Section 3-1 introduces the research question. Section 3-2 provides a literature review. Section 3-3 describes the models. Section 3-4 describes the data. Section 3-5 presents the results. Section 3-6 concludes.

3-1: Introduction

In this chapter, the question of interest is whether politicians who hold powerful positions in Congress channel defense money to their home districts or states. In one respect, this is a well-traveled path. Nevertheless, there is distinct room for not only extension, but also improvement and innovation. The objective of this chapter is to accomplish all three. The extension involves continuing the most recent empirical work, which has examined the political distribution of defense prime contracts through as recently as 1989. Naturally, extending this work involves updating the data set to include the 1990s. This will enable us to see in part whether patterns of politically-motivated defense spending persist into the 1990s, a decade in which there was a dramatic drawdown in defense spending resulting from the collapse of the Soviet Union.

In extending this previous research, a commitment is implicitly made to maintain a structure (i.e., the choice of dependent and independent variables) that closely parallels these previous models and empirical testing. Yet, a deeper examination of the underlying model and hypothesized causal system reveals potential flaws. Therein lies the opportunity for improvement and innovation. The flaw lies primarily in the choice of the dependent variable, namely the distribution by state of defense prime contract awards. First, the use of data aggregated at the state level may confound cause and effect relationships at the congressional district level. For example, consider a state with ten congressional districts. It may be the case that a representative is able to direct contracts to one of these ten districts. In the aggregation of defense spending across all districts into one total for the state, however, the influence of this particular member of Congress may be muted to the point where it does not manifest itself strongly in the regression analysis, if at all. Alternatively, consider the case of an exogenous increase in contract awards for one of the ten other districts. In the aggregation and analysis of the data, a member from the state who sits on a relevant Congressional defense committee would be credited for this increase despite not having played a role in influencing it.[39]

Second, as some critics have pointed out, it may be the case that a subset of defense contracts are not susceptible to Congressional influence owing to either institutional restrictions or more subtle uncertainties regarding the contracting process. Furthermore, prime contract award data does not reflect subsequent redistribution of dollars from prime contractor to subcontractors in other geographical areas. In light of these concerns, the unique contribution of this research is to identify the subset of military spending which can be tracked to congressional district and which historically has been prone to Congressional influence.

In terms of the existing research, Barry Rundquist and a series of co-authors have been the most prominent over the past three decades in testing the hypothesis that defense contracts flow at a higher rate to districts of powerful legislators. Carsey and Rundquist's most recent work (1999a and 1999b) examines the distribution of contracts through 1989, so a natural update to this work is to employ data that includes the 1990s. This extension into the 1990s is of particular interest because of the radical shift in the international

[39] Of course, this would happen if there was an exogenous increase in spending in the member's own

security environment resulting from the dissolution of the Soviet Empire. In addition to extending the data series, this research will employ additional modeling techniques to overcome potential biases in the regression results. One obstacle that Rundquist et. al have confronted is the question of a self-selection bias. That is, politicians are more likely to migrate to committees in Congress in which their home districts have a vested interest, and which have a higher rate of defense spending. The implication is that the hypothesized causal chain is reversed. In other words, the distribution of defense spending explains the geographical make-up of defense committee membership, instead of the other way around. As will be discussed in the literature review, Carsey and Rundquist employ one method to account for this selection bias. This paper will use an alternative approach, specifically the use of a fixed-effect regression model that captures variation over time within the unit of observation; this helps minimize the bias introduced by self-selection.

Rundquist, his co-authors, and a handful of other researchers have fairly consistently employed the distribution of defense contracts to each state as the unit of observation. This research has yielded a mixed bag for finding a systematic political motive in the defense budget. They have found evidence of politicized defense spending to varying degrees, but the evidence is not overwhelming. It is possible that the failure to find a conclusively strong link is an indication that defense spending is not as politically tinged as it is widely perceived to be. It is also possible, however, that the unit of observation (i.e., the dollar value of defense contracts to each state) is not the ideal

district. It is more likely to occur when aggregating spending in multiple districts into one state total.

measure for finding political influence. As discussed in Section 3-1, one criticism is that data aggregated by state-may confound relationships that may or may not be present at the congressional district level. The second criticism lodged primarily by Mayer (1990, 1991) is that the defense contracting process per se is not susceptible to direct Congressional influence. Institutional features, such as the Federal Acquisition Regulations, are such that the military contracting process is immune, relatively speaking, from congressional manipulation. Mayer (1990, 1991) makes the case that it is the subcontracts portion of the defense contracting process that is politically tinged; however, the Department of Defense does not systematically collect subcontract data. While some case studies have examined the politicization of subcontracts for specific defense acquisition programs, it has not been systematically examined because of the lack of data.

This chapter is unique in that it employs a new way to examine the political distribution of defense dollars, which accounts for the aforementioned criticisms. As an alternative to using the state-by-state distribution of defense contracts, the dependent variable considered here is the dollar amount that Congress approves in the military construction appropriations bill each year for projects that were not included in the original Presidential request to Congress. These Congressional add-ons are typically earmarked for particular military installations. These installations can then be easily mapped to a host congressional district to create, in part, a "military pork dataset" at the congressional district level.

The collection and use of this data addresses the flaws. First, the data is collected at the more granular, congressional-district level. Second, the data comes from a process in which the legislators have wider latitude to affect change to their benefit, namely the appropriations process. Furthermore, by focusing on military construction projects, the legislators have a much higher degree of certainty that the money will flow to their district since specific bases are earmarked. In contrast, the flow of defense contract dollars is much less certain. For example, while a prime contractor in a certain congressional district may receive a sum of dollars to build a weapon system, a portion of that money will be subcontracted outside the district.[40] It is also less certain that the dollars sent to prime contractors for weapon systems would necessarily result in capital improvements. This is in comparison to the case of military construction projects in which capital investment is the norm.[41]

Seen in this light, the defense budget is heterogeneous with a line of demarcation between dollars that flow to contractors for weapon system procurement and dollars that flow to specific military installations for capital improvements. It is not the homogenous pot of money implied by models that use the highly aggregated defense contract award data. From the perspective of a legislator trying to win support from the home district, military construction earmarks would seem to be a more certain road to success. The intent of this chapter, in part, is to see whether testing data that comes directly from this more malleable portion of the budget reveals political distribution that may not be readily apparent when considering the defense budget as a homogenous mass.

[40] It would not be as a surprise on a given weapon system procurement if over half of the prime contract dollars are in turn subcontracted out. Mayer (1991) makes this claim.

[41] Note that the earmarked military construction projects will result in the award of construction contracts which would eventually show up in the annual prime contractor data at the state level; however, this is only one part of the state-level prime contract data, which predominantly includes weapon system contracts.

To recap, this chapter contributes to the examination of the presence of political distribution of defense contracts in two ways. First, it extends previous work that has looked at the state-by-state distribution of contracts by considering additional years of data (through 2000 to include the military drawdown period of the 1990s) and by employing techniques to account for a potential self-selection bias in the proposed explanatory variables. Secondly, this chapter creates an innovative point of departure by employing a different measure for the flow of dollars. By using military construction earmarks in the appropriations bill, dollars can be traced to congressional district instead of state.[42] In addition, the subset of the defense budget that may provide more attractive payoffs to legislators is isolated apart from the budget that is either not as attractive or as malleable.

3-2: Literature Review

The literature on distributive politics has several dimensions. A logical starting point is to discuss the general theory of distributive politics and use that as a springboard to discuss more applied work. The applied work can be considered in two distinct subsets. The first is the subset that focuses on the distribution of defense spending. It is rich of and in itself. It is instructive to review it in detail, as it is directly relevant to the questions being examined in this chapter. It is also useful to provide a survey of the second subset, specifically the literature that has focused on examining the distributive

[42] It should be noted here that the potential of a committee member self-selection bias is still present when considering this alternative earmark dataset. Unfortunately, I am not able to control for this bias in the same manner that I use when employing the first dataset, contracts dollars by state. With the state data, I use a fixed effect model to correct for endogeneity. When using the earmark dataset, however, fixed effects estimates are inconsistent because many districts receive no earmarks at all. Intuitively, it is

forces of government spending, exclusive of defense spending. This subset can provide a sense of distributive forces outside of defense and provide alternative techniques to address and measure these forces. These three categories (general theory, applied work in defense, and applied work in non-defense) are discussed in turn below.

3-2.1: Distributive Politics: General Theory

Two papers from the general theoretical literature are described here because they provide an excellent introduction to the underlying theory. Weingast, Shepsle, and Johnsen (1981) have written what is considered the seminal theoretical work. In it, they provide a neoclassical model for distributive politics. They claim that inefficient pork-barrel type projects arise for several reasons. The principal one is that of concentrated benefits and dispersed costs. They say that distributive programs are ones in which the benefits can be targeted to a specific geographic location, unlike entitlement programs (e.g., government pensions) which are geographically dispersed. The second ingredient is that the funding for these targeted programs comes from general level taxation, such that no one particular subset within the population takes a disproportionate hit for footing the bill. Furthermore, the link between the incidence of tax and how that money is spent is not readily known. The result is the production of government projects and programs that are larger than economically warranted.

In general, defense spending would seem to fit well within this theoretical structure. In many circumstances, specific defense contractors or bases can be targeted to

impossible to isolate a fixed effect "portion" of the dependent variable if the dependent variable measures zero. I address this in more detail in the methodology section.

receive particular projects. Furthermore, defense spending is funded by general taxation. In other words, it is not the case that a specific tax is levied each time Congress approves spending for a new defense weapon system.

The second theory-based paper by Shepsle and Weingast (1994) is more properly classified as an historical survey. It tracks the evolution of four theories of the congressional committee structure. Three of these theories are directly relevant here as they explain the role of the committees to be that of either promoting the interests of individual committee members, promoting the interests of the majority party, or promoting the exchange of information regarding government projects.

It is the first two roles (i.e., committees are structured to promote either individual or party interests) with which theories of distributive politics mesh. In distributive theories, the committee structure is seen as the primary vehicle in which members of Congress are able to target benefits such as increased defense contracts, whether those benefits accrue to either the majority party members of the committee or to individual members who sit on influential committees.

The third theorized role of committee runs counter to this. Here, government projects deliberated on by Congress are seen as complex. The benefit of the committee structure is that it allows committee members to specialize in one particular area. The committee structure thus facilitates the exchange of specialized information between committees, for example between agriculture and defense. In this theory, committees are not seen as a vehicle for project targeting, but rather as a vehicle for overcoming the lack of information.

Shepsle and Weingast contend that each model explains some facet of Congress and are not antithetical. They say:

> From an a priori theoretical standpoint, they are not mutually exclusively and may instead represent different and important parts of the same very complex puzzle. Congress is a multifaceted organization, one that is unlikely to be understood in terms of a single principle.[43]

Shepsle and Weingast conclude with a suggestion that future research take a broader, more integrated perspective of the various theories.

Shepsle and Weingast's paper provides an excellent lens for viewing the applied work that is summarized below. For example, one paper tests whether defense contracts flow to majority party members, whereas another tests whether contracts flow to defense committee parties, regardless of party. Yet, attention should be paid to Shepsle and Weingast's warning that these assorted theories not be considered antithetical. To varying degrees, the applied work discussed below is guilty of assuming that the theories are mutually exclusive. One goal of this paper is to place empirical findings for distributive politics in context. I readily acknowledge that a positive finding for one particular hypothesis should not serve as proof that the other theories are discredited.

3-2.2: Distributive Politics: Applied Work in Defense

Rundquist and a series of co-authors have been the most prominent contributors in terms of providing a rigorous analytical treatment in measuring the extent that defense spending is politically motivated. Carsey and Rundquist (1999b) examine whether House or Senate representation on a defense-related committee in the United States Congress

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[43] Shepsle and Weingast (1994), 167

leads to higher levels of defense contract awards for a state or conversely if higher levels of contract awards lead to a higher probability that a state will be represented on a defense-related committee. They consider a cross-lagged effects model in which they regress per capita contracts at the state level on that state's lagged representation on the defense committees. Simultaneously, they regress congressional representation on the defense committees on the lagged value of contract awards. The intent of this procedure is to separate the cause and effect relationships by accounting for the contemporaneous correlation among the error terms of these equations. They state this leads to a set of seemingly unrelated regressions. In essence, this is an attempt to correct for the selection bias so they can isolate the effect that committee representation has on contract awards (i.e., the "committee-induced benefits" hypothesis) and the effect that contracts awards have on committee representation (i.e., the "recruitment" hypothesis). Using data from 1963 to 1989, they find evidence for both hypotheses in the House. That is, in the House, contracts flow to the states where legislators sit in influential positions. In addition, the likelihood that a state will be represented on an influential committee increases as more contracts are awarded to that state. These results did not hold for the Senate.

While Carsey and Rundquist (1999b) do find an effect in the House, they do not address criticism from an institutional standpoint of how it is possible that Congress overcomes institutional restrictions to systematically influence the award of contracts. Conceptually, they do not address the criticism regarding the use of state-level data to draw conclusions about the actions of a representative who represents only a part of the state. In this regard, it seems odd that representatives would be effective in influencing the state-wide distribution of contracts whereas senators would not. Finally, they are unable to account for the fact that prime contract dollar values includes money that will eventually be subcontracted out of the state.

In Carsey and Rundquist (1999a), the authors examine the distribution of contracts from a different perspective. They test two alternatives of distributive politics: a committee-centered distributive theory and a party-centered distributive theory. The committee-centered theory suggests that defense benefits accrue to those who sit on defense related committees, regardless of party. In contrast, the party-centered theory suggests that benefits accrue only to members whose party is in the majority. Analyzing contract data from 1963 to 1989, they find support for the party-centered theory for the entire period in the House and from 1972 to 1989 in the Senate. That is, states with House and Senate democrats (the majority party during the period of observation) sitting on defense-related committees received significant increases in per capita defense contracts. The findings for minority-party republicans were of mixed sign but statistically insignificant. The same criticisms mentioned previously apply here. The authors do not address how legislators are able to overcome institutional restrictions in defense contracting to have this influence; they do not address how to account for subcontracted dollars; and again, the analysis is at the state vice the congressional district level.

In Rundquist, Rhee, Lee, and Fox (1997), the authors study state representation on defense committees in Congress from 1959 to 1989. They examine both general committee representation as well as the differences between democrat and republican representation on defense committees in both the House and Senate. Overall, they find that defense committees are "over-represented" by states that either have a vested interest in defense or that have ideologically conservative coalitions. A state is deemed to have a vested interest in defense by either having a high level of defense contracts or having a large military population, as measured by military payroll. Isolating party representation, they find that defense committees are over-represented by republicans who come from states in which the republican delegation is ideologically conservative or from states that have a vested interest in defense from either a high level of defense contracts or personnel. For democrats, they find that defense committees are over-represented by democrats who come from states in which the democratic delegation is ideologically conservative or from states that have a vested interest in defense from a high payroll, but not from states that have a high level of defense contracts. In other words, they claim that representation by democrats on defense committees is driven more by ideology than by economic interest. This finding is difficult to reconcile with the later findings in Carsey and Rundquist (1999a) in which they find that the majority-party democrats received a higher level of defense contract awards from committee representation.

Rundquist, Lee, and Rhee (1996) provide a relatively simple model of distributive politics in defense spending by regressing per capita defense contracts awards by state from 1965 to 1983 on its lag and lagged representation on defense committees in both the House and Senate. They employ a random effects panel model. They find that representation on a House defense committee at time t-1 translates into a $6.00 increase in state-level per capita contracts at time t. Similarly, membership on a Senate defense committee at time t-1 translates into an \$8.80 increase in state-level per capita contracts at time t. They also find that states in which a member gives up a seat on a House defense-related committee loses a relatively large share of defense contracts in subsequent periods, especially when that member was relatively senior.

Unlike later papers by Rundquist, this effort does not acknowledge the potential of a committee selection bias where representatives conceivably self-select to committees in which their home districts have a large vested interest in defense. This leads to a potential overstatement of benefits (i.e., more contracts) from committee representation. Rundquist (1999b) subsequently acknowledges this criticism. Rundquist, Lee and Rhee also ask why other members of congress not sitting on defense-related committees would allow members on the defense committees to get a larger share of defense benefits. They appeal to theory developed by Weingast and Marshall regarding committee gate-keeping and reciprocal deference to committee members such that benefits to committee-based members will hold over time. This is difficult to reconcile with the later claim of Carsey and Rundquist (1999b) that only majority members on the committees benefit from committee representation, not the entire committee. A final criticism[44] of Rundquist, Lee and Rhee (1996) is that they employ random effects panel model, when a fixed effect panel model might be more appropriate though less efficient and thus less likely to yield statistically significant results.[45]

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[44] The previous criticisms regarding the use of state-level data and institutional barriers to Congressional influence on defense contracts would also apply here though not specifically called out again.

[45] Kennedy (1998), p. 227, for instance, suggests that a fixed effects panel model is more appropriate when there is assumed to be correlation between the error term and the independent variable as would seem to be the case here.

In addition to the work by Rundquist et. al, others have pursued the question of distributive politics within defense. Stroup (1998) considers it by examining the allocation of defense personnel across the states, instead of Rundquist et al's use of the annual dispersion of defense prime contracts. Specifically, Stroup employs the percentage change in personnel assigned to a state over a four-year period as the dependent variable. Stroup uses personnel data from 1964-68 to 1988-92. He finds that states with more senior members, regardless of whether they sit on a defense-related committee, are significantly more likely to gain increased allocation of defense personnel. Also, he finds that states with representatives on a House-related defense committee are significantly more likely to experience personnel gains. The sign for senate membership is positive but not statistically significant. Finally, Stroup finds that states that simultaneously have members on both House and Senate defense-related committees show statistically significant decreases in personnel allocations. He speculates that these states trade from strength within defense to garner benefits for their respective states in other areas.

Stroup does not account for the potential of a committee self-selection bias mentioned previously, which may lead to the overestimation of benefits from sitting on a defense committee. Stroup also does not provide rationale as to the institutional mechanisms that would allow more senior members who do not necessarily sit on defense committee to receive disproportionate increases in personnel. While a positive sign on seniority seems intuitively plausible, it is not clear how the cause and effect relationship actually works. It also begs the question as to why voters would vote out a politician with high level of seniority if seniority plays such a pivotal role in bringing pork windfall to a state.

Ray (1981) uses data from 1968 to 1975 to study the relation between defense contracts, defense personnel, and a state's representation on a defense-related committee. He finds evidence for the recruitment hypothesis in the House of Representatives, that is defense committees are disproportionately composed of representatives from states that have large numbers of military personnel, though not necessarily from states that have relatively high contract awards. Ray also finds evidence that once on the committee, the representatives are able to influence additional personnel and contracts to their districts, especially in the case of personnel. Ray's data runs from 1968 to 1975 and his study is unique from the preceding works in that he maps defense spending to the congressional district level.[46] Ray employs relatively simple statistical techniques (e.g., simple correlation and comparison of averages) to arrive at his conclusions. While he does not employ advanced modeling techniques, his results do shed light on district-level data for two distinct parts of the budget, pay and contracts.

Interestingly, in contrast to his 1981 paper, Ray (1980) does use more sophisticated regression-based models to examine the distribution of both defense and other federal funds from 1969 to 1976.[47] Here, he suggests that the proper unit of observation is the percentage change in federal funding from year to year at the congressional district level as opposed to the gross amount since this better captures the

[46] Ray points out that the data is available at the city and county level, which requires mapping to congressional districts. He says, "The conversion to district-level data was made using the assumption that the distribution of dollars was proportional to its distribution of population." P. 225

marginal change within the state. Studying seven categories of federal spending, he finds no evidence of a systematic influence between political variables and the geographic distribution of federal funds at the congressional district level. Considering both Ray (1980) and Ray (1981), it is not at all clear how his findings can be reconciled. For example, in each paper, he specifically discusses the case of Mendel Rivers, a congressman from South Carolina who was renowned for bringing defense projects to his state. Ray (1980) suggests that Rivers' case is an anomaly and Rivers' ability is more a function of his personality than his power on any particular committee. Conversely, Ray (1981) argues that Rivers is a "...'typical' member of the military committees" who uses power from the committee as much as he possibly can."[48]

Mayer (1991) is critical of political theories of defense spending. He is particularly critical of the research that employs prime contract data as a unit of observation because he questions whether Congress can overcome institutional barriers to directly impact the award of contracts. He points out that empirical studies have not found a relationship between defense contracts and defense committee membership, saying, "Past analyses of defense contracts have never found any relationship between membership on the military committees and higher levels of district defense contract awards."[49] It is surprising that Mayer makes this absolute statement since his work contains a reference to Ray (1981) in which Ray argues that there is a political influence in the award of contracts, although as mentioned above, Ray's findings are ambiguous.

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[47] Although Ray (1981) was published after Ray (1980), it may be the case that Ray (1980) was the more recently-written document as it uses a more recent data series.
[48] Ray (1981), 234
[49] Mayer (1991), 33

At any rate, Mayer offers a comprehensive review of the defense acquisition process and explains the various mechanisms in place that would prohibit systematic intervention by Congress within the formal DoD contracting process. He does explain, however, where Congress does have an influence. Using case studies, he explains that congressional influence typically involves a decision to continue or discontinue funding for a weapon system in which a contract has already been let to a specific contractor. According to Mayer, the best unit of observation for picking up these decision points is not the data that contains the geographical dispersion of prime contract awards but rather the handful of votes that Congress casts each year on whether to continue weapon systems. Here, Mayer claims that while some members of Congress will vote according to parochial interests, most will vote according to ideology. His statistical analysis on the vote of the Martin Amendment for the continued construction of Navy ships in 1987 shows that the overwhelming majority of Congress voted according to political ideology. One objection is the presence of a bias in Mayer's choice of this particular amendment to make this case. There may not be a deliberate bias on Mayer's part but it may be the case that only weapon systems in which ideology is crucial make it to the floor for votes. Nevertheless, Mayer makes the case that these decision points (i.e., specific votes on the continuation of systems) provide a more appropriate venue for testing defense self-interest hypotheses. In general, he claims "Although the procurement process is almost by definition parochial, Congress does a tolerable job at not making major decisions on the basis of purely local considerations."[50]

[50] Ibid, 223

Mayer also points out the use of prime contracts as a measure for the geographic dispersion of contracts is flawed because, "prime contract data reveal nothing about the distribution of subcontracts, which compose over 50 percent of the dollar value of all prime contracts."[51] Finally, Mayer is careful to draw a distinction between different types of defense spending. He acknowledges, for example, the studies that show a correlation between military construction projects (vice major weapon system procurements) and committee representation. He points out that decisions regarding military bases and construction are much different than those involving weapon systems. In large part, the institutional rules are different and allow both Congress and the Department of Defense much more discretion when making decisions about bases, thus opening the door for a much more politically-charged process for military installations.

Mayer (1990) contains similar themes as those developed in Mayer (1991). He provides an overview of the defense source selection process and shows how it is largely immune from direct Congressional influence. He then points to the cases where Congress does have influence in the appropriations process when it approves specific language on the funding of particular programs. Mayer also develops the notion of subcontractor targeting where a prime contractor deliberately distributes subcontracts to the home districts of influential legislators so as to win a broad base of congressional support for the project.

Lindsay (1990) echoes the preceding thoughts from Mayer. He hypothesizes that members of Congress clearly perceive defense spending as heterogeneous. According to

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[51] Ibid, 33

Lindsay, decisions on weapon systems are made according to ideology and decisions on military installations are made according to parochial interests. The rationale is Lindsay's claim that military installations play a more significant role in a local economy than does any one particular weapon system. The politician would pay a higher price in the home district by making a negative decision on a home base that would presumably have a large impact on an economy. Conversely, the norm is for Congress to view decisions on weapon systems as questions of national security and floor debate typically proceeds along these lines. The electoral cost of voting against a weapon system is lower than voting on military base decisions and the benefit (adherence to ideology) is higher. Lindsay provides a survey of studies in the 1970s and 1980s in which congressional voting on a variety of weapon systems is shown to be a function of ideology and not necessarily a function of the legislator's parochial interests. Lindsay does not provide any new statistical analysis but offers an institutional perspective that complements Mayer (1991, 1990).

Markusen et. al (1991) offer an historical account of the development of the defense industry within the United States. They suggest that the geographic dispersion of defense industries forms a belt around the perimeter of the United States. They argue that this "gunbelt" arose from a rather complex historical evolution particularly after World War II to include, for example, certain regions of the country that were more ready to capitalize on advancing the defense technologies of the time. They claim that this historical evolution over-rides any Public Choice theories that explain the geographical distribution of the defense industry through political markets. The authors indicate that politics may play a role at the margin but conclude, "At best, Congress acts as a protector and reinforcer of existing military economies rather than a causal force." [52] At some level, this counters Mayer's (1990) development of a subcontracting theory in which Mayer suggests that prime contractors costlessly locate defense subcontractors to achieve a wide geographical dispersion of work so as to create a strong geographical support in Congress. Even if Mayer were correct, it would appear from Markusen et al's perspective that such a subcontracting strategy would not drastically alter the fundamental concentration of the defense industry. It would take a tectonic shift to do that. In other words, regions outside of the gunbelt may receive defense contracts at the margin but it is unlikely that this work would mean anything substantial to that particular economy, unless there was a deeper fundamental shift in the concentration of the industry. This may help explain why votes on weapon systems that reach a floor vote are decided ideologically. Markusen et al. would claim that the benefits of any particular weapon system have a significant benefit to only a few concentrated regions.

The preceding provides a comprehensive review of the literature on defense distributive politics. The literature itself is not homogenous as it tackles the issue from a number of perspectives. On the one hand, there is the rigorous analytical approach of Rundquist and his co-authors in which they test a variety of theories by using the distribution of defense prime contracts by state as the principle variable to be explained by political factors. They find for political influence using data from the early 1960s through 1989 and by using a variety of regression techniques but their findings are not

[52] Markusen et. al (1991), 242

overwhelming in the sense that some only apply to subsets of Congress while other findings appear to be contradictory. On the other hand, there is a literature that explores and acknowledges the role that institutions have played in the development of the defense industry and its processes. These studies would question empirical findings from Rundquist and others as potentially spurious by pointing out the potential flaw of using prime contractor data distributed by state. Mayer (1991), for example, questions Rundquist's et al use of prime contracts as the best measure but does acknowledge that politics play a factor in other ways such as in subcontracting and funding for military construction projects. Others such as Stroup (1998) and Lindsay (1990) help illustrate the heterogeneous nature of the defense budget by suggesting that political markets play a bigger role when it comes to military installations and personnel vice defense contracts. Finally, Markusen et. al (1991) provide a rich historical reconstruction to explain why they believe that politics play but a small role in the distribution of defense contracts and how the historical evolution of the industry, outside of politics, provides the dominant explanatory case. The preceding studies are the most relevant to the question being pursued in this chapter but other research in distributive politics outside of defense is also important for consideration. This literature is considered now.

3-2.3: Distributive Politics: Applied Work in non-Defense

While defense spending is a rich subject area for distributive politics because it represents a large part of the federal discretionary budget, other areas of government spending have certainly received attention. A sample of empirical work within distributive politics is summarized below and is followed by a brief survey of the literature regarding the general theory of distributive politics.

In a working paper, de Figueirado and Silverman (2001) examine the case of academic earmarks. Earmarks are written by the appropriations committees of either the House or Senate and identify a specific academic institution to receive federal funding. This is attractive to researchers because federal spending can be pinpointed to a geographical point, which then can be easily mapped to a congressional district. In de Figueirado and Silverman, the unit of observation is the dollar amount of earmarks received by a university. The authors examine both the returns to lobbying and whether the institution benefits by being in a district in which there is an influential member on the appropriations committee, after controlling for institutional variables, such as academic quality and student enrollment. They find that lobbying is more effective by institutions when that institution is represented by a member of the House Appropriations Committee (e.g., \$49 to \$55 in additional earmarks for each \$1 spent on lobbying) or Senate Appropriations Committee (e.g., \$18 to \$29 in additional earmarks for each \$1 in lobbying). When an institution is not represented, the returns are just over \$1 in additional earmarks for each \$1 spent in lobbying. The authors provide a rich empirical investigation to include, for example, the use of instrumental variables to control for potential endogeneity in the amount of lobbying. They have, however, more difficulty in explaining why the high returns to lobbying are not dissipated by additional lobbying expenditures. The timeframe they consider, three years over two terms of Congress, is short. It would be interesting to see if their results hold over a longer time horizon.

Knight (2002) examines the distribution of projects in a 1998 Congressional transportation bill. Knight explains how over three-quarters of congressional districts received transportation projects in the bill, significantly more than the minimum majority coalition predicted by standard models. He explains that this is a function of institutional devices (e.g., the presidential veto) that cause the minimum coalition to be greater than a simple majority. Knight then uses the 1998 bill to help simulate the impact of an alternative voting mechanism on the floor, to include for example, the general floor's ability to introduce amendments. The simulation results show that this would further widen the majority coalition, increase spending in districts of the winning coalition, and delay projects, among other things. While Knight's approach is intriguing, he does not address the impact of reciprocity. Even if an open rule is instituted allowing the floor to offer amendments, will a larger coalition necessarily be the result? Or will those on the floor defer to the issuing committee and then expect reciprocal deference on votes in which they have an interest? Also, misleading results may follow from Knight's choice of transportation projects since it would seem that the ability to distribute transportation projects across many districts would be relatively easier than other federally-funded projects. In other words, the size of the winning coalition may in part be a function of the ease of distribution of projects.

Levitt and Poterba (1999) employ a different approach in examining the question of distributive politics. Not only do they examine the connection between congressional representation and the distribution of federal funds, they extend the study to include the link between representation and state growth rates. Using a variety of techniques to control for potential endogeneity in the independent variables, they find that states with more senior congressmen or with a large percentage of politically competitive House districts grew faster on average than other states. They do not find, however, a link between the congressional variables (i.e., seniority, electorally-close districts) and the geographical distribution of federal funds, leaving unanswered the question of the cause of the higher growth. The authors do not offer much as far as alternative explanations as to why there would be such a strong correlation between political variables and growth when the primary instrument available to legislators (i.e., federal spending) is shown to have no effect. It is possible, however, that it is not the level of federal funding that matters with respect to growth in a state but how that money is targeted and how efficiently it is spent, something that the authors are not able to control.

Alvarez and Saving (1997) examine distributive politics within the House of Representatives for the 101st Congress. Using data from the United States Domestic Assistance Programs Database, they examine the distribution of project grants by congressional district. They consider a further refinement by dividing these grants to those whose distribution is dictated by discretion and those whose distribution is dictated by formula. They find a distributive element in overall spending, particularly for legislators who sit on the Ways and Means, Armed Services, Interior, Small Business, and Veterans committees. Interestingly, they do not find a strong influence for either the Agriculture or Public Works committees, which are traditionally considered committees ripe for pork opportunities. They show that districts with close electoral margins did not benefit any differently than districts in which the incumbent was electorally safe. They are also surprised at the result that distributive politics plays a factor in grants dictated by formula.

While Alvarez and Saving (1997) generate interesting results at the district level, there are perplexing institutional questions that may undermine the findings. For example, they discuss how districts of Armed Services Committee members receive a positive distribution of these grants and yet, as they point out, these grants do not even include defense spending. They show how members on the Ways and Means Committee receive significant amounts of agricultural grants and yet those on the Agricultural Committee are impotent in receiving grants of any kind. There lacks an institutional discussion as to why, for example, the Ways and Means Committee is able to influence agricultural grants. As a result, it is not clear if these results are spurious or are reflective of a true causal system. A final criticism is that their explanation as to why formula-based grants are prone to distribution is speculative and not altogether convincing.

Levitt and Snyder (1995) examine distributive politics at the congressional district level. Controlling for a wide range of demographic and socioeconomic variables, they find that the federal distribution of funds from 1984 to 1990 is positively correlated with the number of democratic voters in that district. They also show that particularly senior or influential congressmen are not able to direct federal funds to a specific district. They find that this bias in the distribution of federal funds towards democratic populations holds in the case of federal programs in which the benefits are targeted to specific groups as opposed to large entitlement programs in which the benefits are geographically dispersed. They also say the bias holds when considering federal funds that are allocated by formula because they argue that the formulas would be under greater legislative control. They do not make a strong case, however, in providing rationale explaining why spending dictated by formulas would be subject to such manipulation. In fact, a case could be made that such spending would be harder to control since the formulas have been previously legislated and there would seem to be less room for discretion. An interesting addition to the research would have been to examine if the margin of election victory influenced federal spending in a district.

In *Political Benefits*, Rundquist (1980) offers a collection of papers that examine the question of distributive politics, the more relevant of which are briefly summarized here, specifically the papers of Reid, Anagnoson, and Sylves.[53] Reid (1980) finds that the programs that are more prone to distributive politics are the ones in which there is a political payoff at home. More generally, distributive politics on government projects is, in part, a function of that product's characteristics, for example if administered by formula or bureaucratic discretion. For politically high-payoff projects, Reid finds that politicians are able to effectively direct money towards their districts.

Anagnoson (1980) argues that legislative processes are such that legislators do not necessarily have to sit on an influential committee or possess high seniority in order to obtain benefit. He thus argues for the principle of universalism (i.e., all can benefit) over the distributive theory (i.e., only influential members benefit). He analyzes projects from the U.S. Department of Commerce's Economic Development Administration, which many believe to be susceptible to pork and finds limited proof of distributive politics.

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[53] Ray's (1980a) work from this volume was previously discussed.

Instead, he instead finds that the programs are geographically dispersed, lending credence to the universalism hypothesis.

Sylves (1980) examines the distribution of water projects in New York State from 1971 to 1972 and finds that local community need for the project was the dominant explanatory variable explaining the distribution of projects, although political variables certainly played a role to varying degrees. Sylves' article is unique from others mentioned here by accounting for project priority in terms of legitimate end-user requirements. It is not clear how feasible this approach would be at the federal level, however, as the ability to compare and prioritize end user requirements across a wide geographic spectrum would be difficult. Nevertheless, Sylves' article portrays the provision of public goods at least at the local level as largely responsive to user requirements and that politics plays a role only at the fringe.

In sum, the applied work in both defense and non-defense reveals a mixed bag of results within the realm of distributive politics. The reader leaves the collective mass of reading with no sense of how strong a force the distributive nature of politics is, whether it is illusory or real. For every work that finds a distributive effect, there seem to be just as many that suggest the opposite. The in-fighting among competing theories mentioned by Shepsle and Weingast (1994), is usually subtle but sometimes manifests itself explicitly. For example, Carsey and Rundquist (1999b) conclude their findings for a distributive effect within defense as a denial of Mayer. They say, "This conflicts with Mayer's (1991) assertion that military procurement spending is one of the last places one would expect to find any evidence of distributive politics."[54] Yet, they do not acknowledge, let alone try to overcome, Mayer's (1991) institutional description as to why defense contracts or some subset thereof may be less susceptible to Congressional influence and perhaps why their own results may be spurious. Other research does point to a fruitful approach. Reid (1980) and others suggest that distributive politics play a more prominent role depending upon the type of federal spending or program. de Figueiredo and Silverman (2001), for example, target academic earmarks as prone to distributive spending. This meshes well with the theory of Shepsle, Weingast, and Johnsen (1981) that posits distributive politics are more pervasive when the government programs can be geographically targeted.

The empirical work in this paper will attempt to bridge the disparate findings in the literature. On the one hand, it will continue the work of Rundquist and others by creating a data set similar to theirs, but including additional years and employing additional modeling techniques. Yet, the empirical work here acknowledges the criticism by Mayer that the distribution of prime contracts used by Rundquist et. al is not necessarily the ideal variable to test for defense distributive hypotheses. This paper will thus explore alternative measures and will look to the non-defense literature to provide an approach. Specifically, this paper will follow de Figueiredo and Silverman's (2001) use of Congressional earmarks in academics as a case of targeted spending by considering a comparable case of military construction earmarks.

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[54] Carsey and Rundquist (1999b), 1168

This paper also takes the criticism of Shepsle and Weingast seriously. Regardless of whether a distributive effect is found within defense spending, or a subset thereof, it is important to acknowledge that defense spending and how Congress handles it, is a complex, heterogeneous process. A finding for distributive politics within some realm of the defense process does not mean that the result should be generalized to the entire process. Furthermore, it does not contradict the possibility that other mechanisms have a significant impact in some facets of the defense process. With that being said, it is a good time to proceed into examining the models to be tested here.

3.3: Empirical Models

3.3-1: Distribution of Defense Prime Contracts by State (FY 1963-2000)

Two models will be employed to test for the presence of distributive politics within defense spending. The two models differ primarily in the choice of the dependent variable. Following Rundquist, the first model employs defense prime contract awards by state. Extending Rundquist, the data series runs from 1963 to 2000, an extension of eleven years. Unique is the use of a fixed effects panel model to control for endogenity. The hypothesized equation is as follows:

(Eq. 3.1) $contract_{it} = \beta_1 + \beta_2(contract_{it-1}) + \beta_3(polit_{it-1}) + \beta_4(econ_{it-1}) + \varepsilon_i + \mu_{it}$ where:

i = i-th state and t = t-th year (all states from 1963 to 2000 are considered)

contract = DOD Prime Contract Awards Per Capita (1963-2000)

polit = Separately identified political variables to include:

1) Indicator variables for number of people from state that sit on either the House or Senate Armed Services Committee

2) Indicator variable for number of state members on House and Senate Appropriation committee, subcommittees for defense (available only from 1974 to 2000)

3) Variable to capture the seniority of the representatives

econ = Variable(s) that control for respective state conditions. The primary vehicle to control for these conditions will be lagged gross state product per capita.

μ represents the normal error term

ε represents the unknown but constant parameter to be estimated for each state

The expected signs for the independent variables are as follows. First, it is expected that the lagged value of the contract awards variable will follow the dependent variable closely. Each dollar spent at time t-1 should translate into a positive amount, likely less than a dollar, spent at time t. This controls for the year-to- year "persistence" in contract awards. The variables of interest from a distributive politics standpoint are the political variables. If "benefits" accrue to committee members then the coefficients on both the lagged House and Senate variables should be positive and significant. The coefficients represent the value of having a state member on one of the armed services committees. That is, the presence of a committee member at time t-1 translates into an $X increase in per capita prime contract awards at time t. Thirdly, the expected sign on the gross state product variable is ambiguous. On one hand, it may be positive representing the hypothesis that a state's improved fiscal standing makes it a more attractive place for defense contract awards. Conversely, it may be negative if contract awards are directed towards states in which economic conditions are not as strong, so as to help improve those conditions.

The hypothesized model also includes a state-fixed effect, an unknown but constant effect that is unique for each unit of observation. When a fixed effects model is used, it is assumed that there is a state specific error term that is constant over time. The value of using a fixed effect specification is that it leads to consistent estimates for regression coefficients if one of the independent variables is endogenous. In this particular study, there is concern that membership on the armed services committees is not exogenously determined. Instead, membership may be a function of the state's military production capacity. Use of a fixed-effects approach controls for this by only considering the variation within the unit of observation (e.g., the variation in contract awards for state i over the period). This approach is inefficient because it uses up degrees of freedom by creating a dummy variable for each state. The use of a fixed-effects model in this chapter is unique in the defense distributive politics literature as a way to eliminate bias in the hypothesized political variables.

As indicated, this model follows the spirit of Rundquist et. al's work. In part, the intent is to determine if previous findings for political distribution of defense dollars are robust to both the time period of the data (i.e., this study extends the data through 2000) and the modeling techniques employed (i.e., this study employs a fixed effects model). Since the extension of the data includes the 1990s, there is the added benefit of examining the ramifications of the defense drawdown. Even if evidence of political distribution is found, certain questions persist. For example, does the use of data summarized at the state-level mask more fundamental cause and effect relationships at the district level? Does the use of defense contract aggregated at a high level mask the types of defense spending that are relatively more prone to distribution than others? These questions cannot begin to be answered unless a different tact is taken. Therein lies the objective of the second model that follows.

3.3-2: Earmarked Military Construction Projects (104th-106th Congress)

This second model considers a different dependent variable, specifically the dollar value of earmarked military construction projects received by congressional districts. The hypothesized equation is as follows:

(Eq. 3.2) $$pork_{ct} = \beta_1 + \beta_2(polit_{ct}) + \beta_3(milcap_{ct}) + \beta_4(election_{ct+1}) + \beta_5(term) + \mu_{ct}$$ where:

c = c-th congressional district and t=t-th congressional term (104^{th}-106^{th})

pork = dollar amount of projects designated as pork by the Citizens Against Government using specified criteria

polit = separately identified political variables to include:

1) Indicator variables for number of representatives from congressional district that sit on either the House Armed Services Committee or the House Appropriations Committee, subcommittee for military construction. In addition various breakouts will be considered to include subcommittee chair designators as well as a breakout between majority and minority party members

2) Variable to capture the seniority of the representatives

milcap = the number of military installations within the district

election = the absolute value of the election margin for the upcoming election. For example, if term is 104^{th} Congress, the election margin is for the election results for the 105^{th} Congress.

term = dummy variable for each Congressional term. Dummies are included for the 105^{th} and 106^{th} Congress, with the model intercept assumed to capture the 104h Congress.

As noted, the principle difference between this model and the first is the dependent variable, which considers a subset of military spending (military construction) and is provided at the more granular, congressional district level. It is hypothesized that this subset of the military budget is particularly responsive to political influence because it can be targeted to specific bases, which then can be mapped to specific congressional districts. Several measures of the political variables will be considered, including the general committee membership on both the House Armed Services Committee and the House Appropriations Committee. Separate regressions will consider whether the subcommittees that deal specifically with military construction receive even greater returns. Representative seniority will be considered by employing the log of the number of terms served since it is hypothesized that seniority brings increasing returns at a decreasing rate. The primary control variable will be the number of military installations within the district. It is hypothesized that pork rises incrementally with the number of installations.

This model also includes an election margin variable. This variable represents the closeness of the upcoming election as measured by the winning margin. It is hypothesized that more pork barrel spending will be employed when the margin in the impending election is expected to be close. This implies an expected sign of negative on the election variable coefficient since elections that are not close (i.e., increased margin) would be expected to yield less pork.[55] Finally, term dummies are included for each term.

The differences between this model and the first model extend beyond how the dependent variable is defined. First, note that the first model relied heavily on lagged independent variables whereas this model does not. One benefit of considering this type of data as opposed to the use of contract data in the first model is that the payoff to a representative for earmarking funds to a military installation during the appropriations process is immediate. Conversely, if a legislator did influence the more general category of military contract awards, that influence would not be felt until a later term because of the lead time between Congressional approval and contract award. At this point, cause and effect relationships are difficult to substantiate.

Second, this model only considers the case of representatives as compared to the previous model's use of representatives and senators. Since the data is now provided at the district level, I believe that representation in the House is the best explanatory variable. It is more difficult to pinpoint systematic behavior of a senator attempting to channel funds to a specific congressional district since most senators are responsible for multiple districts.

The third difference involves the assumed form of the model. In the first model, each state received some amount of contract awards and this amount changed from year to year. In this second model, many districts receive no pork whatsoever. Considering

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[55] Note that the first model did not include an election margin variable because it was not clear what margin would be the most appropriate to use. The unit of observation of the first model is at the state level, where a wide number of elections are possibly at play including the senators as well as representative from all districts. This would lead to potentially ambiguous results.

the shorter time span (i.e., 6 years or 3 terms), there are many instances where there is no variation in the dependent variable. In this case, the data is censored at zero. The model best suited for handling censored data is the Tobit model.[56] In particular, I will use the "xttobit" procedure in the Stata statistical software package as this procedure accounts for the fact that the dataset has a panel structure (i.e., cross-sectional, time-series data) with the pork received by each district measured for three successive Congressional terms.

Another pitfall of using a dataset in which the data is censored is that if a fixed effects panel treatment is used, it will degenerate.[57] Intuitively, if a particular district records no pork in three successive terms (i.e., there is no variation in the dependent variable), it is impossible to isolate a district-specific fixed contribution apart from the marginal effects of the right hand side variables. Recall that the use of a fixed effects form was the form suggested in the first model to control for potential endogeneity of the independent variables. Nevertheless, the potential of endogeneity is present since it is expected that representatives will self-select to committees in which their district has a vested interest. This would cause an upward bias in the estimated returns to committee membership. It is hypothesized that this bias will be nonetheless tempered by including a control variable for the number of military installations within the district as a measure of military capacity.

In sum, the two hypothesized models in this section serve several purposes. On the one hand, the first model stays within the spirit of the existing empirical research on

[56] See for example Johnston and DiNardo (1997), 436-441

[57] On page 183, Baltagi (1995) discusses how the fixed error will not be swept away and as a result of the inconsistency in the fixed error term, the regression coefficient for the independent variable and the normal error term cannot be estimated consistently.

the distributive politics of defense spending. It does so by studying the political distribution of defense prime contract awards by state. This model is unique from the currently available work in that it has expanded both the data series and use of modeling techniques. Despite the fact that it is used here, the employment of aggregate contract data at the state level in the first model is questioned as to whether it is the best measure in which to find political influence. Accordingly, a second model is developed which examines the political distribution of military construction earmarks by congressional district. The hypothesis is that these earmarks are particularly responsive to political distribution, controlling for district-specific factors. In general, the second model is developed to help address the criticisms of the first model. In so doing, this chapter redirects the empirical testing of the distributive politics of defense spending.

3.4: Data

This section describes the data and its sources.

1) Prime contract award data are available in the annual Department of Defense Almanac and re-published annually in the Statistical Abstract of the United States. The dollars are listed as current dollars and thus must be converted to constant dollars using the consumer price index. For this effort, current dollars are converted to constant FY96 dollars. Note that contract awards are identified by government fiscal year. Prior to 1977, the government fiscal year ran from July to June. In 1977, the fiscal year was redefined and slipped by three months from October to September. There was a three

month transition period (July-September, 1976) resulting from this re-definition.[58] These three months are not included in the data.[59]

The type of contracts that this data includes is varied. Included are contracts for procurement, research and development, services, construction, supplies, and equipment. Note, therefore, that contract money originates from two separate appropriations, defense and military construction. As pointed out previously, the fact that a contract has been awarded to a particular state does not mean the contract work will actually be accomplished in that state since some portion of the work will be subcontracted. There is no comprehensive database that identifies the magnitude or distribution of subcontract work and this is a pitfall of using this data to find for distributive effects. For specific information on how state contract dollars are accounted for, refer to the Department of Defense, Washington Headquarters Service, Directorate for Information Operations and Reports.

2) In the second model that is considered, the dependent variable is military "pork" received at the congressional district level from FY 1995 to FY 2000 within the military construction appropriation. This information is taken from the Citizens Against Government Waste (CAGW), a non-partisan group which publishes an annual pork book that is available on-line at www.cagw.org. CAGW uses the following criteria to identify pork-barrel spending. An appropriation qualifies as "pork" if it meets at least two of these:

[58] That is, FY76 data includes contract awards from July 1975 to June 1976 and FY77 data includes contract awards from October 1976 to September 1977.

[59] The Statistical Abstract indicates that the data includes the three-month transition period; however, I consulted the DoD office responsible for the data and confirmed that it does not include these three months.

a. Requested by only one chamber of Congress;

b. Not specifically authorized;

c. Not competitively awarded;

d. Not requested by the President;

e. Greatly exceeds the President's budget request or the previous year's funding;

f. Not the subject of congressional hearings; or

g. Serves only a local or special interest.

Some subjectivity enters in this assessment, particularly in the last criteria though the fact that the appropriation must meet at least two of the criteria tempers this criticism. Regardless, the potential of a selection bias should be noted though it is not likely that a bias would be significant.

For the military construction items cited as pork, a military installation is typically identified as a recipient. These installations were in turn mapped to a congressional district, primarily using a military installation to congressional district mapping provided in *Congressional Districts in the 1990s* composed by Congressional Quarterly, Inc. (1993). If a military installation was spread across two districts, the amount of pork was simply divided in half and allocated evenly to each district.

3) Gross state product figures are available from two sources. The Bureau of Economic Analysis (Department of Commerce) has one measure for the years 1977 to present on their website at www.bea.gov. An article by Renshaw, Trott, Jr. and Freudenberg in the *Survey of Current Business* (May 88) identifies GSP from 1963 to 1986. For the ten years in which the data from these two sources overlap (i.e.,1977-

1986), the measures track closely. The figures are not identical, but they are not off by some identifiable fixed amount or percentage either. For this research, I combined the two sets by using the figures from the *Survey of Current Business* through 1976 and the figures from the website of the Bureau of Economic Analysis from 1977 to present. I convert to constant dollars using the Consumer Price Index.[60]

4) Annual population estimates for each state are provided at the Bureau of Economic Analysis website. Population figures are used to change gross dollar values for the various measures to per capita estimates.

5) Membership on the various committees is available from several different sources. One source is the annual *Congressional Directory*, which details not only committee membership but also subcommittee membership.[61] Nelson and Bensen's (1993) *Committees in the U.S. Congress:1947-1992* offers a concise record of committee membership and detail such data as committee seniority, but does not offer subcommittee information.

6) Data specific to congressional districts is available from a dataset made available by Professor Scott Adler at the University of Colorado.[62] Adler's data is available by Congressional term through 1998, the 105th Congress. It includes a wide range of economic and demographic data, the most relevant for the purposes of this chapter are population and the number of military installations per each district. Note

[60] The Bureau of Economic Analysis cautions against converting current to constant dollars for long time stretches (e.g., converting 1960 current dollars to 1996 constant dollars) because the fidelity of the numbers will be lost.

[61] Subcommittee membership is only detailed from the mid 1970's; prior to that, only subcommittee chair information is readily available.

that it was necessary to extend Adler's data series by one term to include the 106th Congress. To extend the series, the population data was simply duplicated from the 105th to the 106th Congress. The number of military installations by district and congressional term required a closer examination since a redistricting in 1998 resulted in the change of districts for some military installations for the 106th Congress.[63]

7) Election results for the House of Representatives for each Congressional District are provided in the Statistical Abstract of the United States.

8) Other political data such as majority/minority splits in Congress are summarized at http://userwww.service.emory.edu/~erein/data/Index.html#Congress.[64]

3.5: Results

3.5-1: Model One (Distribution of State Prime Contract Awards, FY63-00)

The results provided in Table 3-1a are provided from the regression that considers the influence of committee membership on the distribution of defense contract awards. This helps answer the question of whether benefits (i.e., defense contracts) flow to the districts of defense committee members, regardless of whether the member is in the majority. For the entire time period (1963-2000), complete membership information is only available for the House and Senate Armed Services Committee.[65] In column (1), the results from this entire time period are provided. As expected, the coefficient on the lag

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[62] The dataset can be downloaded from http://sobek.colorado.edu/~sadler/districtdatawebsite/CongressionalDistrictDatasetwebpage.html.

[63] The only districts in which military installations were redistricted for the 106th Congress were in North Carolina. The 1st District picked up one installation and the 7th District picked up four installations. The 3rd and 8th districts lost installations in this redistricting.

[64] I collected a wide range of summary-level data to have at my disposal for future research.

[65] Membership from the relevant subcommittees of Appropriations is available from 1974 to present and this will be considered in subsequent regressions.

of contract awards is positive and significant. This coefficient implies that for every one dollar of prime contracts awarded at t-1, 68 cents will persist into the following period. This is lower than the estimates provided by Carsey and Rundquist (1999a) that suggests that 90 cents of every dollar will carry through. This difference results from the use here of a fixed effects model in which the state-specific dummy variable absorb some of the persistence, thus attributing the persistence instead to a fixed state characteristic.

The coefficient on the lag of gross state product (-.003) is statistically significant. It implies that for every dollar increase in per capita gross state product, the per capita defense contracts will decrease by three-tenths of a cent. This suggests that contracts are directed to states in which overall state economic performance is lacking as opposed to regions in which the economy is doing well.

Interestingly, none of the political variables enter significantly. The coefficients on House committee membership and seniority (-0.72 and 4.77 respectively) are not statistically significant. Similarly, the coefficients on Senate committee membership and seniority (0.022 and –1.16 respectively) are not statistically significant either. This result is interesting in light of previous findings for political distributive effects using similar data through 1989.

In light of these previous findings, the regressions were re-accomplished considering the data sets in two periods, 1963 to 1989 and 1990 to 2000. These results are provided in columns 2 and 3 of Table 3-1a, respectively. Several interesting shifts manifest themselves between the two periods. In the 1963-1989 data set, the coefficients on House and Senate committee membership imply that committee membership translates into an additional $15.00 and $14.02 in per capita defense contract awards respectively. This offers some evidence of political distribution as in the similar studies; however, neither coefficient is statistically significant, though the House coefficient is marginally significant. In the 1990-2000 data, membership on the Senate Armed Services Committee gains in both magnitude (41.66) and significance (better than 10 percent). Membership in the House, however, is negative (-13.86) although House seniority plays a positively significant role. This suggests that those in the house have had diminished influence in the 1990s. The other interesting shift is with respect to gross state product, which goes from marginally positive (0.001) in the 1963 to 1989 period to significantly negative (-0.007) in the latter period suggesting that defense contracts have become increasingly directed towards performing states in the 1990s.

Note that the use of the fixed effects model in this analysis is stringent in that it uses more degrees of freedom since a dummy variable is created for each state. A less stringent approach would be to employ a random-effects model, which does not dummy out each state. In addition to being less stringent, the random effects model is also less appropriate if there is a concern regarding the correlation between the error term and one of the independent variables as is the case here between committee membership and the error. Arising from its relative efficiency, the random effects model is more likely to provide statistically significant coefficient estimates than the fixed effects treatment.

For example, Rundquist, Lee, and Rhee (1996) used a random effects model. While they find a statistically significant relationship between committee membership and defense contracts, the results in this paper for a similar data series are less conclusive owing in part to the employment of the more stringent technique. To help illustrate this, the results from Table 3-1a are re-stated in Table 3-1b, with the exception that the results are generated from a random-effects model in lieu of the more appropriate fixed effects model. Two items of interest are mentioned here for illustration. In the random effects model using the 1963-1989 data, the magnitude (18.06) and significance (5% level) of the estimates for returns to House membership are greater than the magnitude (15.00) and significance (10% level) generated by the fixed-effects approach. Similarly for the gross state product variable, the magnitude (0.002) and significance (10% level) are greater from the random effects model as compared to the estimate (0.001) and significance (less than 10% level) from the fixed effects estimate. In each example, potential bias in the regression coefficients is reduced or eliminated by use of the fixed effects model.

In addition, I conducted a Hausman test to compare whether the two forms of the model (i.e., fixed effects and random effects) produced similar results. If the results are statistically similar, the random effects model is preferable because it is more efficient. Otherwise, the fixed effects model is preferable because it produces consistent estimates in light of what is presumed to be endogenity in the independent variables. I ran the Hausman test to compare the coefficients from each column in Table 3-1a and Table 3-1b respectively. For example, the coefficient of 0.68 on lagged contracts in column (1) of Table 3-1a was compared to the coefficient of 0.89 in column (1) in Table 3-1b. In each case that I ran the Hausman test, I found that the fixed effects and random effects estimates were statistically different and I thus rejected the null hypothesis that they were the same. Since the fixed effects model is conceptually more appropriate, the estimates generated from it are more reflective of the underlying causal system. More generally, this is one area of improvement to the literature that this paper offers.

The results in table 3-2 repeat the results in Table 3-1a but the independent political variables have been expanded to include membership on the relevant defense subcommittees of the House and Senate Appropriations Committee. Since I was able to obtain subcommittee membership only from 1973 to present, the results in table 3-2 reflect fewer observations. The three trends that were present in the results from Table 3-1a continue to hold here. Specifically House committee members appear to lose influence in the 1990s; Senate committee members gain influence in the 1990s; and defense contracts are directed more toward poorer performing states in the 1990s as compared to the 1974-1989 period.

Several potential reasons emerge to explain the trends in both the overall period (1963-2000) and in the two sub-periods (1963-1989 and 1990-2000). For example, the "benefit" of house committee membership drops substantially from the earlier period ($15 in additional per capita contract awards for a committee seat) to the later period ($13.86 reduction in per capita awards for a committee seat). This may be a result of the peace dividend period of the 1990s in which districts that had received disproportional gains in earlier times were now taking disproportional hits. On the other hand, the benefit of Senate committee membership rises in the later period, suggesting perhaps that the Senate exercised more influence in the contract award process during the drawdown period of the 1990s. A study of the institutional details might reveal other ideological or structural shifts that would explain the apparent loss in influence of House committee members in the 1990s and the increasing power within the Senate. It may also shed light on why defense contracts were apparently used as an equalizing economic force in the 1990s, as compared to earlier periods.

Yet, any such explanations would be plagued by the criticisms mentioned throughout. Specifically, Mayer (1991) would ask how the defense contracting process, as measured by the distribution of aggregated dollars, could be so manipulated in the first place, let alone what may have changed in the 1990s to enable these specific shifts within the House and Senate to occur. As someone who has worked in the defense acquisition industry throughout this period, I am not aware of any changes in acquisition policy in the 1990s that would make Congressional influence more or less likely. Other concerns involve the ability to identify a causal chain in the highly aggregated defense contracting data. Whereas a plausible causal relationship may be drawn for a subset of the contract data, it is increasingly difficult to draw relationships when the data reflects a heterogeneous mix of processes and funds. Implicit in this criticism is that the findings here and in past studies are spurious. I respond to this criticism by identifying a subset of military spending which can be gathered at a detailed level and which represents a part of the budget that Congress can more effectively target. This is addressed in the following section.

3.5-2: Model Two (Distribution of Military Construction Earmarks, 104th-106th Congress)

In this section, the case of earmarks in the military construction appropriation process is taken up. The use of earmarks as the dependent variable is a response to the criticism that the data tested in the first model and elsewhere is too highly aggregated by type (i.e., defense contracts are considered a homogenous mass of spending) and by geography (i.e., defense spending is analyzed at the state level). First, earmarks are a direct and immediate result of the appropriations process within Congress. Specifically, Congress approves funding for special interest projects or projects not requested in the President's budget. Thus, the hypothesized cause (membership on committee) and effect (earmarked projects) occur within the same period. This is unlike the previous case in which the passage of time, as reflected by the use of lagged independent variables, between cause and effect increases the likelihood that a spurious or ambiguous result will emerge. Furthermore, the characteristic of military construction projects is such that the earmarks can be specifically identified to a military installation and thus to a congressional district. Analysis can then proceed at the district level, instead of at the state level, as in the first model.

An appropriate starting point is to provide a sense of the magnitude of the earmarks within the overall defense budget. This is summarized in Table 3-3 for the years 1995 to 2000 for both the defense budget and the military construction budget.[66] The data in Table 3-3 reveal that the military construction appropriation is much more susceptible to targeting in terms of the percentage of the appropriation that is earmarked. Whereas only 1.8 percent to 4.3 percent of the defense appropriation is targeted, anywhere from 7.6 percent to 41.2 percent of the military construction budget is targeted for earmarks. Note the outlying case of the 41.2 percent of earmarks in the FY 2000

[66] Recall that these two categories are handled by two separate appropriations bills.

military construction budget. For FY 2000, the President proposed a scaled-down construction budget that included only partial funding of certain projects, whereas the norm is that each project is fully funded. Congress rejected this proposal and restored full funding, apparently because they were not convinced by DoD's promise that full funding would be provided in subsequent budget requests. According to the criteria used by the Citizens Against Government Waste, the fully funded projects technically qualify as pork-barrel spending. Even if an alternative figure is used,[67] the percent of earmarks is roughly 12 percent. The fact that a higher percentage of this military construction budget is earmarked as compared to other parts of the defense budget supports the hypothesis that the military construction budget is a relatively desirable target for special interest projects for members of Congress.

Table 3-4 presents a profile of the districts that received military construction earmarks by term of Congress. The data is divided to show those districts that were represented on the committees and those that were not. For example, for the 105th Congress, if a district was not represented on a committee, it averaged \$2.4 million in earmarks whereas those that were represented averaged \$8.2 million. The null hypothesis that these means are the same is rejected at the 1% level, for not only the 105th Congress, but the 104th and 106th Congresses as well. Admittedly, the average for the districts with no representation is influenced by a proportionately larger number of districts that receive nothing. By conditioning the sample to focus only on districts that receive a positive sum, the respective averages are \$10.8 million representing 75 observations for districts

[67] Senator John McCain identified approximately \$1 billion of the FY00 budget as questionable special-

that are not represented on a relevant committee and $16.6 million representing 53 observations for districts that are represented. These summary level statistics provide initial support for the hypothesis that those districts represented on defense-related committees receive higher levels of earmarks. Regression analysis follows in which controls are used so that the particular variables that influence these earmarks are isolated and a more comprehensive picture can emerge.

Table 3-5 summarizes the results from the regression of earmarks by district on a political variable (i.e., committee membership), seniority, election margin, and number of military installations. Three regressions were run and the results are provided in columns (1) through (3). In each case, a random effects form of the Tobit model is used. The three regressions differ in the choice of the political variable. In column (1), membership on either the House Appropriations Committee or the House Armed Service Committee is included. In the second, only subcommittee chairs are included. This includes subcommittee chairs of either of the two aforementioned committees. Note, for example, this includes the 13 subcommittee chairs in the House Appropriations Committee, 11 of which are not defense-related chairs. In the third run, only the military construction subcommittee members from the two respective committees are included.

First, consider the results in Table 3-5, column (1). As expected, the control for military capacity, the number of military installations within the district, is strongly and positively related to the amount of earmarked projects that a district receives. Each installation means an additional $14.54 in additional earmarked projects. After

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interest projects, or approximately 12 percent of the FY00 budget..

controlling for military capacity, a strong political influence is present. Specifically, membership on a defense-related committee in the House translates into an additional $13.89 in per capita military construction earmarks. This is significant at the one percent level. Note that the coefficient on committee representation ($13.89) is quite similar to that on the military installation control ($14.54) providing a relative sense of the power of committee membership.

Conversely, representative seniority does not provide an explanatory role; the coefficient on seniority is negative but not statistically significant. This suggests that committee membership is the overriding influential political variable in explaining earmarks. Once on a committee, a representative can influence the appropriations process, regardless of how long he or she has served.

Another interesting result is in the election variable, which has a negative coefficient (-0.10), as hypothesized. The coefficient means that for every one percent increase in the winning margin of the impending election, there is a 13-cent decrease in the per capita construction earmark. This is significant at the one percent level. This suggests that earmarks are used not only in a distributive sense, but also as a tool to influence close elections.

The results in column (2) and (3) reflect similar models, but with alternatively specified political variables. Specifically, the results in column (2) reflect the substitution of the indicator variable for general committee membership with an indicator variable for subcommittee chairmanship. The coefficient on this variable (11.58) is positive and statistically significant. It drops in both magnitude and significance as compared to the more general committee membership variable. The coefficients for the other variables are stable in that they do not change appreciably in light of the change of the independent variable for committee membership.

The results in column (3) reflect the inclusion of an indicator variable for only those members who serve on the military construction subcommittees of either the House Appropriations Committee or the House Armed Services Committee. The coefficient on this variable (\$18.46) shows the relative power of the subcommittee members to influence the earmarking of military construction projects, about \$4 more than the influence of those at the general committee level (\$13.89) and thus shows that subcommittees wield influence. The coefficients on the remaining variables do not change appreciably from the first two regressions.

The preceding results were generated using the xttobit command in Stata. This command generates a Tobit estimate with a random effects model assumed, using the congressional district as the unit of observation. As a double-check, I also ran a procedure within Stata called interval regression. Interval regression is a generalization of the Tobit procedure in which an interval is defined for the censored region as opposed to the Tobit procedure in which a specific point is identified as the censoring point.[68] The advantage of the interval regression procedure is that it allows robust standard errors to be estimated to account for the fact that earmark data from the same district may not be independent from one term to the next. In table 3-6, I compare the coefficients, standard error and z-score of the three committee variables of interest. There are differences in the

[68] See http://www.stata.com/support/faqs/stat/tobit.html

magnitude of the estimated coefficients, with the xttobit procedure providing the smaller estimated coefficients in magnitude in these cases.[69] The significance level of each coefficient, however, remains approximately the same, not only for this selected subset of coefficients but in general across the board.

An alternative way of considering the model depicted in Table 3-5 is revealed in the results of Tables 3-7 and Table 3-8. The hypothesized variables remain the same but the regression is accomplished on data that has been conditioned on whether the district receives any positive sum of earmarks. This conditioning is done in two parts. The first part (Table 3-7) involves replacing the dependent variable (dollar value of per capita construction earmarks by district) with an indicator variable. This variable assumes a value of zero if the district received no construction earmarks and a value of one if the district received any positive amount of earmarks. The coefficients from this first part can be interpreted to mean what a one unit increase in the level of the independent variables means with respect to the probability that the district will receive earmarks in the first place. The second part (Table 3-8) then involves conditioning the sample and running the regression for only those districts that do receive a positive amount of earmarks and then studying the impact of the hypothesized variables for the subset of the data. In essence, the first part reflects the process of what it takes to "get in the game", so to speak. The second part then explains the influence of the hypothesized variables once "in the game".

[69] This results from the fact that xttobit utilizes the panel structure of the data and a random effects specification.

Some interesting results emerge from considering the results in Table 3-7 and Table 3-8 in tandem. First, note that in the previous results (i.e., Table 3-5), the evidence pointed to the relative power of the subcommittee. Members from the relevant subcommittee generated a significant volume of earmarks for their constituencies ($18.46 per capita) relative to other members in the committee structure ($13.89 and $11.58 per capita respectively). The results in these two tables provide a deeper picture. Table 3-7 shows that subcommittee membership significantly increases the probability that the district will receive earmarks, by about 21 percent. This is greater in magnitude than either subcommittee chairs (10 percent) or committee members in general (also 10 percent). Yet, once the data has been conditioned on only districts that receive positive level of earmarks, a different story emerges. Table 3-8 reveals that the power of subcommittee members to attract earmarks ($9.59 per capita) is now quite comparable to the power of the more general committee membership ($9.01 per capita) and to subcommittee chairs ($9.52 per capita). Thus, the advantage of subcommittee membership in terms of receiving earmarks is the increased probability of benefiting in the first place, not in the subsequent level of benefits. One possibility this suggests is that subcommittee members seek to improve the likelihood that their earmarked projects will receive general committee approval by also recommending earmarks that would benefit powerful subcommittee chairs as well as the general committee membership.

Another interesting result involves the election margin. The previous results in Table 3-5 suggested that close elections in a district mean more earmarks in that district. Tables 3-7 and 3-8 further suggest that while close elections increase the probability that a district will receive a military construction earmark as shown in Table 3-7, close elections do not translate into increased levels of earmarks, as shown in Table 3-8. Intuitively, this makes sense. If only the incumbent candidate has the ability to use the earmark, the use of earmarks as a political tool will likely not escalate. Furthermore, even if the incumbent wanted to escalate the amount, the appropriation process is not conducive to it since earmark opportunities arise at a couple of discrete points in the appropriations process as opposed to being a continuous, open process.

Note that the election margin used here is from the results of the upcoming election. The dollar value of earmarks that are awarded during the 105th Congress, for example, is regressed against the election margin from the 106th Congressional elections. Intuitively, this makes sense in that a member of Congress will take action to seek earmarks in the current term so as to improve chances in the upcoming election. As a point of comparison, however, the model summarized in Table 3-5 was re-accomplished by instead using the margin from the election that immediately preceded the current term. In Table 3-5, it was shown that the coefficient on the election margin for the impending election was either –0.10 (column 1), -0.10 (column 2), or –0.15 (column 3) depending on what independent variables were used. Each estimate was statistically significant at the 5% level or better. If the models are run using the election margin from the most recent election instead of the upcoming election, these coefficients would be -0.07, -0.07, and –0.08 respectively and none would be statistically significant at the 10% level or better. This lends credence to the idea that members of Congress are attempting to position themselves for the upcoming election as opposed to acting in response to previous election margins.

Also note that seniority continues not to play a statistically significant role in either increasing the probability (Table 3-7) or increasing the level of earmarks (Table 3-8) that a district receives. In fact, seniority has a slight dampening effect on the probability that a district will receive earmarks, but this result is not statistically significant.

As a final point of interest, one alternative to the model examines whether the majority party has an advantage over the minority party in obtaining earmarks. I specifically consider the model in which general committee membership is separated into majority and minority membership. The results are provided in Table 3-9. In column (1), I consider the case where the dependent variable is earmarked projects by district for the entire dataset. In columns (2) and (3), I consider the conditioned data. Specifically, in column (2), the dependent variable is a 0-1 dummy variable, with a value of one meaning that the district received some positive level of earmarks. In column (3), the dependent variable is the amount of earmarks for only those districts that receive the amount. In each case, I test the coefficients on the majority and minority party and am unable to reject the null hypothesis that there is no difference between the two coefficients. In other words, it appears that the majority party does not have an advantage over the minority party in obtaining earmarks.

In sum, the preceding results suggest that controlling for district-level military capacity, there is a distributive element to military construction projects. The model described in this section is simple from a mathematical modeling standpoint. Despite the simplicity, it is robust to the criticism lodged against the first model in which the results were questioned as spurious. Specifically, the cause-effect link between committee representation and earmarked projects is considerably less ambiguous than the previous link between committee representation and the aggregated category of defense contract awards. Subsequently, the conclusions drawn here have a stronger foundation.

While a handful of authors have suggested from a hypothetical and institutional standpoint that military construction projects are particularly prone to pork-barrel spending, this is the first effort to test the hypothesis systematically and subsequently to find evidence for it. Considering that the hypothetical literature presents a convincing case of why military construction spending provides such an attractive target, the empirical results that follow here, in general, are not altogether surprising. Yet, some surprising sub-plots manifest themselves. In particular, as discussed immediately above, members of the relevant subcommittee have a significant advantage over other members of the same committee in increasing the probability of earmarks but not necessarily the level of earmarks. Similarly, earmarks are used as a tool in close elections but the use appears to be a one-shot event as opposed to an escalatory tactic. Finally, representative seniority does not play a significant role in the distribution of military construction earmarks. These nuances are not picked up in the previous hypotheses and their revelation here is a benefit to this empirical approach.

While the results here are promising, the hypothesis and modeling are certainly not above criticism. First, it is possible that in forming the defense budget, the DoD officials purposely leave out projects which are valid military requirements, but which they know are also the pet projects of certain legislators. The thought is that the legislators will eventually add the projects regardless and this allows the DoD to fund other projects. This leads to an overstatement of the general level of pork-barrel type spending because these add-ons are automatically considered 'wasteful' by definition, despite being valid requirements. It also should be noted, however, that the DoD may include certain pet projects in their budget request to facilitate the passage of the appropriations bills and this would lead to an understatement of the general-level of pork-barrel spending. A deeper institutional study is required to examine the prevalence of these practices. A deeper statistical analysis is also possible and would call for the examination of the complete military construction budget, not just the subset that was added by Congress. For the purpose of this chapter, however, the concern is simply noted.

A second criticism would involve that of committee self-selection in which representatives self-select to committees in which their districts have a vested interest and this leads to increased spending in that district and biased results for the effect of committee representation. Controlling for military capacity within the district reduces this bias and thus the estimated returns to committee membership are over and above what the military capacity would tend to dictate. With these findings and criticisms so noted, it is an appropriate time to conclude.

3-6: Conclusion

The intent of this chapter has been to re-connect the theory and the empirical testing of distributive politics of military spending. The theory suggests that distributive spending occurs when the money can be targeted to a specific group. Within the empirical portion of the defense distributive literature, however, it is not at all clear that the most recurring choice for the dependent variable (i.e., prime contract awards by state) adequately represents a pot of money that can effectively be targeted. While some subsets of these dollars may be targeted, the aggregation of the data to include dollars that cannot be may mask specific effects.

What is interesting is that some authors have nonetheless found a distributive effect using this as a dependent variable. Considering this, this paper has extended the time series of the data used in these previous studies and found that effects that were present in the 1960s through the 1980s no longer appear to hold in the 1990s. It is unclear, however, whether this represents a fundamental structural shift in distributive defense spending or if it points to the fact that the results generated by using this aggregated data are spurious or are otherwise ambiguous.

Not wanting to leave an aura of ambiguity, this chapter pursues this question by seeking a way to tighten the link between the theory and the empirical testing. A new dependent variable is employed, namely the amount of military construction earmarks that a congressional district receives. It is not too surprising that the tests using this data reveal a statistically significant distributive element. What is surprising are some of the attendant story lines such as the negligible impact of seniority and the role of earmarks in districts with a close electoral vote.

The importance of earmarks to the legislators can also be supported using anecdotal evidence. In the FY98 military construction appropriations bill, President Clinton used the newly awarded presidential line-item veto power to strike 38 projects worth $287 million from the military construction budget approved by Congress. Congress apparently viewed this as an assault on their ability to generate projects of special interest to their constituencies. The House voted 347 to 69 and the Senate voted 78 to 20 to override the veto.[70] These numbers are overwhelming considering that only a handful of districts benefited from these 38 projects. It offers a strong signal that legislators saw this as a fight not only for these particular construction projects but for their ability to generate pork-barrel projects in all appropriations. After the military construction veto, the President retreated on line-item motions he had made in other appropriations. In the end, he was able to block a total of less than $200 million from the entire federal budget.[71]

Another interesting anecdote comes from the case of Representative Ed Royce from California during the deliberations on the FY96 military construction bill. Royce fashioned himself as a "pork-buster" and argued strongly against a particular aspect of that bill which he considered wasteful and bad precedent. Yet another legislator immediately criticized Royce by pointing out that Royce had surreptitiously requested

[70] CQ Almanac, 1999, p 2-82
[71] Ibid.

funding for a special project that Royce claimed would help him in his home district.[72] The point is that even some of those who rail against 'wasteful' spending do not hesitate to avail themselves of opportunities to engage in it because the practice is an accepted norm within Congress.

The military construction appropriation offers a particularly nice venue for a systematic study of distributive politics because it is relatively easy to map the recipient of the earmark to its respective congressional district. There is no reason to believe that the effects found in the earmarks for this appropriation would not play out similarly in earmarks for other appropriations, for example in the separate appropriation for defense. Unfortunately, the defense budget does not lend itself as well to a large-scale mapping of earmarks to district as is in the case of military construction. But if it were possible, it is not unreasonable to expect similar forces at play.

That being said, the claim is made earlier that the reader leaves the collective literature on distributive politics confused as to its magnitude. The implicit suggestion here is to use a barometer such as the earmark listings by the Citizens Against Government Waste to give a sense of the magnitude. It is expected that the level of pork-barrel spending within any one appropriation is a function of the percentage of that appropriation that can be targeted to specific projects. Consider the case of the military construction appropriation a rough upper bound. It is easily targetable and its percentage of special earmarked projects has averaged about 10%.[73] Of course, this figure is quite

[72] CQ Almanac, 1995, p. 11-67
[73] That excludes the anomaly of the FY00 budget.

debatable[74], but it does provide a sense of distributive politics as playing a role for a portion of the budget. It certainly does not preclude the possibility that other theories of Congressional action (see criticism of Shepsle and Weingast, 1994) can be functioning simultaneously in other areas of the legislative domain.

This paper has been positive in approach. There is a separate normative question as to whether distributive politics constitutes a public problem that somehow should be negated. Proponents of the Presidential line-item veto, for example, assumed "that members would be loathe to vote for a handful of congressional add-ons once a president had isolated them as wasteful projects."[75] Yet, as discussed in the example of the FY98 military construction appropriation, Congressional overturn of the President's line item veto of 38 projects suggests that Congressional ability to manufacture pork is firmly ensconced. In any event, a full normative treatment is left for subsequent discussion.

In closing, this chapter has both extended and provided a new direction within the literature on the distributive politics of defense spending. While the first part of this effort extended the data and analysis from prior research, this approach is simultaneously criticized for its failure to link the theory with the empirical design in testing that theory, which leads to a potential for ambiguous results. An alternative approach is then offered and focuses on projects, in accord with the theory, which can be easily targeted. A definite distributive element is found for this subset of projects and the implication is that

[74] It has been implicitly assumed here that the initial budget request for military construction from the President represents valid military requirements and is largely immune from political manipulation. A separate study would be required to see if this is the case. If a distributive element is found in this core request, the 10% figure is an understatement.
[75] CQ Almanac, 1998, 2-82

distributive politics is clearly an explanatory variable for at least a portion of defense-related spending.

Table 3-1a: Distribution of Prime Contract Awards by State (Fixed Effects Model)

	Dependent Variable is Per Capita Contract Awards by State		
	(1) 1963-2000	(2) 1963-1989	(3) 1990-2000
Lagged Contracts (Log Per Capita Contracts at t-1)	0.68*** (41.52)	0.56*** (25.80)	0.51*** (16.83)
HASC Member (at t-1)	-0.72 (0.09)	15.00* (1.86)	-13.86 (1.41)
HASC Seniority (log years at t-1)	4.77 (0.70)	4.89 (0.52)	16.31* (1.73)
SASC Member (at t-1)	0.02 (0.00)	14.03 (0.55)	41.66* (1.91)
SASC Seniority (log years at t-1)	-1.17 (-0.13)	-14.56 (-1.12)	16.21* (1.73)
Gross State Product (at t-1)	-0.003*** (3.94)	0.001 (1.02)	-0.007*** (3.03)
State Fixed Effect?	Yes	Yes	Yes
No. of Observations	1850	1300	550
R^2	0.83	0.81	0.85

1. Unit of observation is Per Capita Contract Awards by State
2. Considers only representation in the House and Senate Armed Services Committee. The results in Table 3-2 are broadened to include representation in the House and Senate Appropriations Committee.
3. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 3-1b: Distribution of Prime Contract Awards by State (Random Effects Model)

	Dependent Variable is Per Capita Contract Awards by State		
	(1) 1963-2000	(2) 1963-1989	(3) 1990-2000
Lagged Contracts (Log Per Capita Contracts at t-1)	0.89*** (91.35)	0.88*** (70.47)	0.86*** (54.16)
HASC Member (at t-1)	12.02* (1.91)	18.06** (2.06)	4.86 (0.77)
HASC Seniority (log years at t-1)	-2.84 (0.50)	-5.82 (0.76)	6.41 (0.97)
SASC Member (at t-1)	10.81 (0.60)	-2.35 (0.10)	48.23** (2.39)
SASC Seniority (log years at t-1)	0.59 (0.07)	6.14 (0.56)	-10.15 (1.10)
Gross State Product (at t-1)	0.003*** (0.34)	0.002* (1.87)	0.001 (1.43)
State Random Effect?	Yes	Yes	Yes
No. of Observations	1850	1300	550
R^2	0.83	0.81	0.90

1. Unit of observation is Per Capita Contract Awards by State
2. Considers only representation in the House and Senate Armed Services Committee. The results in Table 3-2 are broadened to include representation in the House and Senate Appropriations Committee.
3. A Hausman test was conducted to compare the coefficients from the fixed effects model in Table 3-1a to the coefficients from the random effects model in this table. The null hypothesis that the coefficients from each model are the same is rejected at the 1 percent-level for each column of results.
4. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 3-2: Distribution of Prime Contract Awards by State (FY 1974-2000)

	Dependent Variable is Per Capita Contract Awards		
	(1) 1974-2000	(2) 1974-1989	(3) 1990-2000
Lagged Contracts (Log Per Cap Contracts at t-1)	0.73*** (38.35)	0.53*** (18.06)	0.51*** (16.86)
Defense Member in House (at t-1)	-12.08* (1.70)	6.73 (0.59)	-11.79 (1.47)
Defense Seniority in House (log years at t-1)	7.40 (1.10)	12.99 (1.21)	5.60 (0.66)
Defense Member in Senate (at t-1)	14.71 (1.05)	15.13 (0.72)	25.34 (1.54)
Defense Seniority in Senate (log years at t-1)	7.40 (1.10)	-1.61 (0.15)	-6.00* (0.64)
Gross State Product (at t-1)	-0.005*** (4.91)	0.004** (2.11)	-0.007*** (2.97)
State Fixed Effect?	Yes	Yes	Yes
No. of Observations	1350	800	550
R^2	0.86	0.85	0.84

1. Unit of observation is Per Capita Contract Awards by State
2. These results differ from those provided in Table 3-1 because the independent variable has been broadened to include representation on defense subcommittees in the House and Senate Appropriations Committee. This subcommittee data was only readily available from the mid 1970's.
3. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 3-3: Earmarks as % of Total Appropriation FY 95-00 ($96M)

	Defense	Mil. Construction
FY95 Appropriation	$243,724,188	$8,836,000
FY95 Earmarks	$4,492,072	$901,898
% Earmarks	1.8%	10.2%
FY96 Appropriation	$243,406,197	$11,177,009
FY96 Earmarks	$10,453,139	$854,511
% Earmarks	4.3%	7.6%
FY97 Appropriation	$244,177,588	$9,982,309
FY97 Earmarks	$8,997,176	$930,084
% Earmarks	3.7%	9.3%
FY98 Appropriation	$247,708,522	$9,183,248
FY98 Earmarks	$8,560,825	$921,390
% Earmarks	3.5%	10.0%
FY99 Appropriation	$250,510,548	$8,449,742
FY99 Earmarks	$6,151,039	$1,032,169
% Earmarks	2.5%	12.2%
FY00Appropriation	$267,795,360	$8,374,000
FY00 Earmarks	$8,976,799	$3,451,615
% Earmarks	3.4%	41.2%

Table 3-4: Summary-level, Dollar Value of Earmarked Projects ($96M)

	104th Congress (1)	105th Congress (2)	106th Congress (3)
1. Not Represented on Defense Committee	$2.8M	$2.4M	$5.34M
(# of observations)	325	322	315
2. Defense Committee Member	$6.7M	$8.2M	$16.8M
(# of observations)	110	113	120
3. Appropriations or Armed Service Subcommittee Chair	$7.1M	$7.7M	$26.1M
(# of observations)	20	20	20
4. Armed Services or Appropriations Military Construction Subcommittee Member	$7.2M	$14.2M	$26.8M
(# of observations)	28	30	30

1. A statistical test was conducted to compare the mean of earmarks received in each term for those districts that are not represented on a defense committee (Row 1) and those districts that are represented (Rows 2-4). The null hypothesis that the means are the same is rejected in each test at the 1% level.

Table 3-5: Distribution of Earmarked Military Construction Projects by Congressional District (Tobit Regression with Random Effects)

	Dependent Variable is Per Capita Earmarked Projects by District		
	(1)	(2)	(3)
# Military Installations	14.54***	15.13***	14.88***
	(12.72)	(13.73)	(16.79)
Election Margin (margin at t+1)	-0.10**	-0.10**	-0.15***
	(1.94)	(1.89)	(2.97)
Committee Member	13.89***		
	(4.05)		
Subcommittee Chair		11.58*	
		(1.68)	
Subcommittee Member			18.46***
			(4.13)
Rep. Seniority (Log # of terms)	-1.09	-1.06	-0.96
	(0.62)	(0.59)	(0.55)
105^{th} Term Dummy	-1.46	-1.40	-1.74
	(0.55)	(0.53)	(0.65)
106^{th} Term Dummy	16.88***	17.20***	17.00***
	(6.80)	(6.93)	(6.83)
Random effects?	Yes (District)	Yes (District)	Yes (District)
No. of Observations	1304	1304	1304
Chi-2 Sig Level	0.00	0.00	0.00

1. Unit of observation is dollar value of earmarked military construction projects by congressional district.
2. "Committee member" refers to anyone sitting on either the House Armed Services Committee or the House Appropriations Committee.
3. "Subcommittee chair" refers to anyone who chairs a subcommittee in either the House Armed Services Committee or the House Appropriations Committee. This includes 11 of the 13 chairs in the Appropriation subcommittee that are not defense-related.
4. "Subcommittee member" refers to anyone who sits on the military construction subcommittee for either the House Armed Services Committee or the House Appropriations Committee.
5. z-statistics in parentheses
*=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 3-6: Distribution of Earmarked Military Construction Projects by Congressional District (Xttobit versus Interval Regression)

	Dependent Variable is Per Capita Earmarked Projects by District	
	Xttobit	**Interval Regression**
Committee Member		
Coefficient	13.89***	18.78***
Standard Error	3.43	4.32
z-score	4.05	4.35
Subcommittee Chair		
Coefficient	11.58*	16.81
Standard Error	6.88	6.79
z-score	1.68	1.04
Subcommittee Member		
Coefficient	18.46***	32.07***
Standard Error	4.47	5.19
z-score	4.13	6.19
Robust standard error	No	Yes
No. of Observations	1304	1304
R-squared (overall)	0.18	0.17

1. Unit of observation is dollar value of earmarked military construction projects by congressional district.
2. This table reflects a subset of the results from regressing earmarks on the various specified variables. In column (1), the results are generated from the xttobit procedure in Stata. In column (2), the results are generated using interval regression with robust standard error specified
2. "Committee member" refers to anyone sitting on either the House Armed Services Committee or the House Appropriations Committee.
3. "Subcommittee chair" refers to anyone who chairs a subcommittee in either the House Armed Services Committee or the House Appropriations Committee. This includes 11 of the 13 chairs in the Appropriation subcommittee that are not defense-related.
4. "Subcommittee member" refers to anyone who sits on the military construction subcommittee for either the House Armed Services Committee or the House Appropriations Committee.
5. z-statistics in parentheses)
*=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 3-7: Distribution of Earmarked Military Construction Projects by Congress (Binary Dependent Variable: 0: no earmarks ; 1: earmarks)

	Dependent Variable is Binary (0 if District received no earmarks; 1 otherwise)		
	(1)	(2)	(3)
# Military Installations	0.12***	0.12***	0.12***
	(10.27)	(10.51)	(10.70)
Election Margin	-0.002***	-0.002**	-0.002***
(margin at t+1)	(3.37)	(3.41)	(3.59)
Committee Member	0.10***		
	(3.11)		
Subcommittee Chair		0.10	
		(1.46)	
Subcommittee Member			0.21***
			(4.39)
Representative Seniority	-0.001	0.000	0.006
	(0.04)	(0.02)	(0.04)
105^{th} Term Dummy	-0.02	-0.02	-0.02
	(1.08)	(1.03	(1.02)
106^{th} Term Dummy	0.05***	0.06***	0.06***
	(2.45)	(2.57)	(2.54)
Random Effects	Yes (District)	Yes (District)	Yes (District)
No. of Observations	1304	1304	1304
R-squared (overall)	0.18	0.17	0.19

1. Unit of observation is 0-1 binary variable with value of 1 if positive amount of military construction projects received by congressional district and 0 if none received
2. "Committee member" refers to anyone sitting on either the House Armed Services Committee or the House Appropriations Committee.
3. "Subcommittee chair" refers to anyone who chairs a subcommittee in either the House Armed Services Committee or the House Appropriations Committee. This includes 11 of the 13 chairs in the Appropriation subcommittee that are not defense-related.
4. "Subcommittee member" refers to anyone who sits on the military construction subcommittee for either the House Armed Services Committee or the House Appropriations Committee.
5. t-statistics in parentheses
*=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 3-8: Distribution of Earmarked Military Construction Projects by Congress (Conditioned on Districts that receive positive level of earmarks)

	Dependent Variable is Per Capita Earmarked Projects by District		
	(1)	(2)	(3)
# Military Installations	6.84*** (10.28)	7.11*** (10.68)	7.19*** (10.95)
Election Margin (margin at t+1)	-0.009 (0.17)	0.001 (0.03)	-0.01 (0.20)
Committee Member	9.01*** (3.47)		
Subcommittee Chair		9.52* (1.92)	
Subcommittee Member			9.59*** (2.73)
Representative Seniority	-0.21 (0.13)	-0.67 (0.41)	-0.12 (0.08)
105^{th} Term Dummy	1.76 (0.55)	1.76 (0.55)	1.13 (0.35)
106^{th} Term Dummy	18.22*** (6.05)	18.22*** (6.05)	17.83*** (5.95)
Random Effects	Yes (District)	Yes (District)	Yes (District)
No. of Observations	439	439	439
R-squared (Overall)	0.30	0.29	0.30

1. Unit of observation is dollar value of earmarked military construction projects by congressional district, conditioned on those districts that receive some positive amount.
2. "Committee member" refers to anyone sitting on either the House Armed Services Committee or the House Appropriations Committee.
3. "Subcommittee chair" refers to anyone who chairs a subcommittee in either the House Armed Services Committee or the House Appropriations Committee. This includes 11 of the 13 chairs in the Appropriation subcommittee that are not defense-related.
4. "Subcommittee member" refers to anyone who sits on the military construction subcommittee for either the House Armed Services Committee or the House Appropriations Committee.
5. t-statistics in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 3-9: Distribution of Earmarked Military Construction Projects by Congress (Majority versus Minority Party Committee Membership)

	D.V. is Earmarks by C.D.	D.V. is 0-1 Binary (=1 if Earmark Received>0)	D.C. is Earmarks by C.D. conditioned on positive amount
	(1)	(2)	(3)
# Military Installations	14.51*** (12.58)	0.116*** (10.24)	6.84*** (10.24)
Election Margin (margin at t+1)	-1.01** (1.91)	-0.002*** (3.37)	-0.008 (0.15)
Majority Party	15.03*** (3.49)	0.105** (2.49)	9.27*** (2.97)
Minority Party	12.60*** (4.52)	0.096** (2.29)	8.67** (2.50)
105th Term Dummy	-1.48 (0.56)	-0.024 (0.280)	1.72 (0.54)
106th Term Dummy	16.86*** (6.79)	0.053*** (2.44)	18.12*** (6.07)
Random Effects	Yes (District)	Yes (District)	Yes (District)
No. of Observations	1304	1304	439
R-squared (Overall)	N/A	0.18	0.30
Wald Test for Model Sig.	Significant	Significant	Significant

1. Unit of observation is military construction projects by congressional district. In column (1), this is total dollar value of the projects. In column (2), the dependent variable is a 0-1 binary variable with value of 1 if positive amount of military construction projects received by congressional district and 0 if none received. In column (3), the depended variable is total dollar value of projects received by districts, conditioned on those receiving a positive amount.
2. "Majority Party" refers to anyone sitting on either the House Armed Services Committee or the House Appropriations Committee from the majority party.
3. "Minority Party" refers to anyone sitting on either the House Armed Services Committee or the House Appropriations Committee from the majority party.
4. In each of the three model runs, I am unable to reject the null hypothesis that the coefficients on the majority and minority membership variable is the same.
5. t-statistics in parentheses
*=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Chapter 4: Conclusion

This chapter summarizes the findings of Chapter Two and Chapter Three. In addition, I consider directions for future research. Section 4-1 considers the baseball stadium research of Chapter Two. Section 4-2 considers the defense spending research of Chapter Three.

4-1: Baseball Stadium Findings and Future Research

The research in Chapter Two contained two objectives. First, I sought to extend the literature on the economics of sports stadiums to include the interests of the professional athlete by quantifying the extent to which athletes financially benefit from playing in a new stadium. Second, assuming that a new stadium effect on salary in fact exists, I sought to identify the source of that benefit. Does player salary improve in a new stadium because the player's marginal productivity has improved or because new stadium rents are being shared from owner to player? I consider the particular case of baseball players since, among other things, baseball offers a multitude of performance statistics as well as a deep economic literature that has developed models for estimating salary and marginal productivity. In addition, baseball has witnessed a spike in stadium construction over the past decade. Furthermore, most of the stadiums have been publicly funded and baseball's success in getting public funding owes in part to an antitrust exemption it has had since the 1920s. In this regard, a publicly funded stadium is considered to be a monopoly-like rent.

The production function of a baseball player is assumed to be that of winning games. Using data from 1990 to 1999, I show that new stadiums do not enhance the ability of a team to win games and if anything, a new stadium has a dampening effect on winning percentage, holding playing talent constant. I use this evidence in part to conclude that a new stadium does not alter a player's marginal productivity. Nevertheless, the data reveals that pitchers receive a new stadium-induced hike in salary from 13% to 30% for pitchers depending on what specification of a salary model is used. Since a pitcher's marginal productivity in a new stadium does not increase, I conclude that these hikes result in large part from rent sharing between owner and players.

The case for batters is less clear. Including a dummy variable for a new stadium in a regression of individual batter salary on performance reveals a positive finding (13%) on the new stadium dummy variable in only one of four specified models. I then add an interaction term of stadium and performance in the individual salary model as one way to separate the marginal productivity effects of stadium on salary, as measured by the coefficient on the interaction term, and the rent sharing effects, as measured by the coefficient on the new stadium dummy variable. In this test, I find that rent sharing is not present. On the other hand, when individual batter MRP is calculated and included into the individual salary models for batters, the coefficient on the new stadium variable is significant in three of four models and suggests rent sharing in the range of 16% to 27%. It should be noted that calculation of individual MRP is a crude process and its use may yield spurious results. In any event, pitchers appear to have an advantage over batters in attracting rents. A possible explanation for this is offered within the chapter.

The research in Chapter Two marks a unique contribution for a number of reasons. Of the readily available literature on the economics of sports stadiums, it is the first to quantify the impact of stadium construction on the athlete. This research is also unique in pursuing the question of rent sharing. In contrast to the existing literature on rent sharing, empirical testing using baseball affords the ability to control for individual performance that is not available in the studies to date. Furthermore, whereas previous studies used deregulation as the event to test for rent sharing, this research uniquely considers the reverse direction. That is, this paper shows that rent sharing occurs after the monopoly owner makes use of that power to obtain a rent. In this case, the rent is a new stadium.

There are several opportunities for further research in this area. The first involves a deeper examination of the way in which these rents are transmitted to the players. Do the spikes in salary occur roughly at the same time for all players? Or are they first transmitted to free agent players since a team is able to use its relative revenue advantage, resulting from a new stadium, to attract free agents by signing them to a salary that is higher than the player's previous performance would otherwise warrant? The hypothesis would then be that the signing of a free agent in turn initiates a new salary scale for the entire team and all players eventually benefit, but gradually do so over time.

In a similar vein, it would be interesting to see if the salary determination process in baseball is such that the estimates for rent sharing in this chapter are underestimated.

For example, consider a team with a new stadium that is able to sign a player to a salary that is higher than performance indicators would otherwise warrant. Does this new salary then re-define the salary-performance scale such that players from all teams will seek a salary commensurate from their respective teams for similar performance indicators, regardless of whether that team has a new stadium? If players are in fact successful in doing so, it is possible that they are benefiting from a new stadium salary effect despite not playing on a team with a new stadium. In this case, the estimates in Chapter Two for rent sharing are underestimated. This may also explain why teams without new stadiums place increasing pressure on local communities to get new stadiums. A closer look at the salary negotiation process would help sort this out.

A second area for expansion of this research would involve testing for rent sharing in other professional sports such as football, basketball or hockey. Basketball offers a potential foil to baseball because unlike baseball each team is faced with a cap on the amount of salaries it can pay. It would be interesting to see if these caps preclude the possibility of rent sharing altogether or if perhaps rent sharing still occurs, but at some lower level.

A third area for research would involve a re-examination of Public Choice models of rent-seeking coalitions and rent-seeking costs. In light of this research and other studies that find evidence for rent sharing, does it become necessary to extend the Public Choice models of rent-seeking to include not only the efforts of the monopoly owner (or prospective owner) but also to the factors of production who seek employment in the monopolized field?

Other opportunities for research in this area certainly exist. Sports offers a particularly fertile ground for hypothesis development and testing, not only for its ample supply of data, but also for the dynamics of the relationship between the firms (i.e., the team), the players, government officials, and the public. This research taps into only one small aspect of these dynamics but yields rich results.

4-2: Defense Spending Findings and Future Research

The research in Chapter Three contains two different tracks for the testing of distributive politics within defense spending. The first tracks follows the path established by a series of researchers who have examined the distribution of defense contract dollars using a data series that starts in 1963. The common characteristic of this track lies in the choice of the dependent variable, the distribution of defense prime contracts by state. Using this variable, researchers have proposed a host of political variables ranging from Congressional seniority to defense subcommittee membership to explain the distribution of defense contract awards.

Chapter Three uniquely contributes to this research track by extending the data series to include the 1990s and by employing additional techniques (specifically the use of a fixed effects model) to control for endogeneity of the explanatory variables. The principal finding is that trends in the distribution of contracts that were present through 1989 no longer appear to hold in the 1990s. Members of the House of Representatives appear to lose power in the 1990s whereas members of the Senate appear to gain in power. It is hypothesized that this may be the result of a change in the defense environment resulting from the collapse of the Soviet threat in the 1990s.

While I extend this existing track of research in Chapter Three, I draw upon other literature in this area and simultaneously criticize this approach and subsequently create a second track. I levy the criticism against the choice of the dependent variable in the first track of literature, including the first part of Chapter Three of this paper, as being too highly aggregated in two respects. First, the aggregation of the data by state may mask cause and effect relationships at the congressional district level. Second, the aggregation of the data to include all types of defense contract spending from weapon procurement to military construction may mask cause and effect relationships that may be present in only one subset of the spending. Considering this criticism, I seek a dependent variable that can be tracked to the district level and which is by and large represents a homogenous subset of the overall defense budget.

The second track of research in Chapter Three is thus uniquely characterized by the choice of the dependent variable. The variable chosen is the dollar value of Congressional earmarks arising from the appropriations process of the military construction budget. This variable has the nice characteristic that the spending can be pinpointed to a particular military installation and thus to a particular congressional district. Using data from the 104^{th} through 106^{th} Congress, I find that members of the House of Representatives are able to significantly influence the flow of earmarks to their home districts. Members of the relevant military construction subcommittees (i.e., from the House Appropriations Committee and the House Armed Services Committee) hold a particularly strong advantage in obtaining earmarks in that there is a higher probability that their districts will receive an earmark as compared to other committee members.

Conversely, representative seniority appears to play no role in the distribution of earmarks. There also appears to be an electoral incentive in the distribution of earmarks as districts in which the election margin is close have a higher probability of receiving earmarks. Finally, the majority party does not hold a statistically significant advantage over the minority party in receiving earmarks.

As is the case with the research on sports, there is ample room for additional research in this area. With respect to the findings from the first track of research in Chapter Three, a deeper institutional study would reveal the extent to which there was a structural shift away from the House to the Senate in terms of the two sides to influence of distribution of contracts. A failure to find a structural shift would point to the possibility that the findings are spurious owing to the use of data that is too highly aggregated.

Regarding the second track of research, Chapter Three took an agnostic stance on whether a political influence exists in the distribution of the military construction budget that is formed prior to the process in which the Congressional earmarks are added. Since the earmarks typically represent only about ten percent of the military construction budget, this leaves a sizeable amount of the budget that may be subject to political influence. Research in this area would then involve a rather massive undertaking of pinpointing the spending within this part of the budget to see whether similar patterns emerge.

While Chapter Three makes the case that the military construction budget is a homogenous subset contained within the overall heterogeneous mix, further research would entail examining the rest of the defense budget to see what other subsets could likewise be considered homogenous and “targetable” to see if similar influences exists. Of course, this could be extended beyond defense as already done by de Figueiredo and Silverman’s (2001) consideration of earmarks arising from academic grants.

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Appendix 2.1, Sources of Data

Item	Source[76]	Comment
Salaries (1990-99)	Fort Website	1. See http://users.pullman.com/rodfort/ 2. Fort identifies original source newspaper dates that list annual player salaries
Team Revenue Data (1990-1996)	Daniel Rascher	1. Professor Rascher provided these data via e-mail on a spreadsheet 2. Original source is *Financial World* Magazine; the data was published annually, typically in the May-June timeframe
Team Revenue Data (1995-1999)	Levin et. al (2000)	See *The Report of the Independent Members of the Commissioner's Blue Ribbon Panel on Baseball Economics*
Team & Individual Performance Data	*Baseball Database 4.0* (2000)	See www.baseball1.com for ordering information
Local Area Population	Bureau of Economic Analysis	Available at www.bea.gov.

[76] The source of these data refers to the source from which I directly received the data. In some cases, these are secondary sources but I assume that these sources accurately transcribed the data from the original or primary source.

Appendix 2.2: Sensitivity of Stadium Age

In the analysis of Chapter Two, the baseline case is that a stadium is considered new if less than five year old. Some of the models from Chapter Two were re-run considering two alternative lengths of stadium age, less than seven years old and less than ten years old. The results are summarized for a subset of the variables below in Tables 2.2-1 through 2.2-4.

The analysis summarized in Table 2.2-1 repeats the analysis from Table 2-8 in Chapter Two in which team revenue is regressed against a variety of variables, including a dummy variable to indicate whether the team has a new stadium. Table 2.2-1 summarizes the subset of results that show how team revenue responds to a new stadium depending on the varying definitions of stadium newness. Even if a "new stadium" is re-defined, the effect that the stadium has on team revenue not only remains strong but actually strengthens, from 22.4% (baseline case) to either 29.2% (stadium is "new" if less than 7 years old) or 25.7% (stadium is "new" if less than 10 years old).

The analysis summarized in Table 2.2-2 repeats the analysis from Table 2-9 in Chapter Two in which team winning percentage is considered to be a function of on-field performance. The two variables of interest here are the interaction terms between the dummy variable for the new stadium and the variable for either batting or pitching. These coefficients help measure the extent to which a new stadium impacts team batting or pitching performance. In Chapter Two, the empirical evidence shows that a new stadium has a slightly negative impact on team performance. These are the results reprinted in the first column of Table 2.2-2 and this is what was offered as evidence that a new stadium does not positively impact the marginal productivity of players and if anything has a negative impact. I speculated that this might result from the fact that it takes time for a team to adjust to a new stadium. Indeed, it appears that if a new stadium is redefined to be either less than seven years old or less than ten years old, it is still case that a new stadium does not alter the marginal productivity of players. The negative impact shown in the baseline case, however, is not as strong in either magnitude or significance once the definition of new stadium is altered. This is shown in the second and third columns of Table 2.2-2.

The analysis summarized in Table 2.2-3 and Table 2.2-4 repeats the analysis from Table 2-4 and Table 2-5 in Chapter Two in which various salary models are presented for batters and pitchers respectively. The variable of interest here is the dummy variable for new stadium. The coefficient on this variable measures the percentage increase in a player's salary when that player plays on a team with a new stadium, regardless of whether that increase is driven by a change in marginal productivity or rent sharing. In the first alternative (i.e., second column of Table 2.2-3 and Table 2.2-4), the effect that a new stadium has on either batter or pitcher salary increases in magnitude and significance when the newness of stadium is re-defined to be less than seven years in all four salary specifications. In this case, batters are shown to benefit from a new stadium by 2.3% to 20.0% and pitchers are shown to benefit from 18.5% to 35.3%. This result suggests that the impact of a new stadium is longer lasting than expected but coincides with the finding in Table 2.2-1 in which new stadiums continue to have a strong impact on team revenue over time. This impact does diminish in the second alternative when a new stadium is redefined to be less than ten years old. In this alternative, the new stadium impact on salary decreases in magnitude for all cases for both batters and pitchers and drops in significance for all but one case. This may be interpreted to mean that the novelty of the new stadium finally drops off at about ten years and the attendant impact on salary also diminishes. It is also possible that some of the effect on salary is diminished as part of the salary effect of new stadiums may be transmitted to players on teams who do not play in new stadiums.

Table 2.2-1: Sensitivity of Analysis to Stadium Age (Team Revenue)

	Dependent Variable is Log Team Revenue		
Effect (New Stadium if < X Years)	**Baseline Case (<5 years)**	**Alt. 1 (<7 years)**	**Alt. 2 (<10 years)**
New Stadium (=1 if Stadium is New, 0 otherwise)	22.4%*** (7.10)	29.2%*** (9.25)	25.7%*** (8.63)

1. Unit of observation is Team Revenue per Year
2. The analysis in this table repeats the analysis from Table 2-8, column (4). I regress the log of team revenue on regional population, team winning percentage and its lag, an indicator variable for each season, and an indicator variable for whether the team plays in a new stadium. The results in this table specifically show the coefficient on the new stadium variable and how this coefficient responds to a change in how a new stadium is defined (<5 years old, <7 years old, and < 10 years old).
3. t-statistics are in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2.2-2: Sensitivity of Analysis to Stadium Age (Team Winning Percentage)

	Dependent Variable is Team Winning Percentage		
Effect (New Stadium if < X Years)	**Baseline Case (<5 years)**	**Alt. 1 (<7 years)**	**Alt. 2 (<10 years)**
New Stadium*Runs Scored	-0.0000155 (0.92)	-0.0000122 (0.45)	-0.0000116 (0.76)
New Stadium*ERA	0.011 (0.99)	0.08 (0.77)	0.007 (0.66)

1. Unit of observation is Team Winning Percentage per Year
2. The analysis in this table repeats the analysis from Table 2-9. I regress team winning percentage on runs scored, earned run average, and interaction variables between runs scored and new stadium and earned run average and new stadium. The results in this table specifically show the coefficients on the two interaction terms to show how these coefficients respond to a change in how a new stadium is defined (<5 years old, <7 years old, and < 10 years old).
3. t-statistics are in parentheses

   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2.2-3: Sensitivity of Analysis to Stadium Age (Individual Batter Salary)

	Dependent Variable is Log of Individual Batter Salary		
Effect (New Stadium if < X Years)	**Baseline Case (<5 years)**	**Alt. 1 (<7 years)**	**Alt. 2 (<10 years)**
New Stadium (Classic w/o fixed effects)	3.9 (0.94)	8.7%** (2.31)	4.8% (1.41)
New Stadium (Classic w/ fixed effects)	-0.05% (0.95)	2.3% (0.42)	-0.02 (0.33)
New Stadium (Alternative w/o fixed effects)	-0.01% (0.22)	2.4% (0.53)	-0.02% (0.50)
New Stadium (Alternative w/ fixed effects)	13.6%*** (2.04)	20.0%*** (3.12)	10.8%* (1.81)

1. Unit of observation is the log of individual batter salary
2. The analysis in this table repeats the analysis from Table 2-4. I regress the log of individual batter salary on total bases lagged, experience variables, and an indicator variable for whether the batter played on a team with a new stadium. The results in this table specifically show the coefficients on the new stadium indicator variable to show how this coefficient responds to a change in how a new stadium is defined (<5 years old, <7 years old, and < 10 years old).
3. t-statistics are in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Table 2.2-4: Sensitivity of Analysis to Stadium Age (New Stadium Effect on Individual Pitcher Salary)

	Dependent Variable is Log of Individual Pitcher Salary		
Effect (New Stadium if < X Years)	**Baseline Case (<5 years)**	**Alt. 1 (<7 years)**	**Alt. 2 (<10 years)**
New Stadium (Classic w/o fixed effects)	21.8%*** (4.06)	23.7%*** (4.78)	20.2%*** (4.52)
New Stadium (Classic w/ fixed effects)	13.2%* (1.91)	18.5%*** (2.81)	17.0%*** (2.86)
New Stadium (Alternative w/o fixed effects)	20.9%*** (3.07)	20.8%*** (3.32)	16.4%*** (2.90)
New Stadium (Alt. Model w/ fixed effects)	30.0%*** (3.53)	35.3%*** (4.38)	30.9% (4.22)

1. Unit of observation is the log of individual pitcher salary
2. The analysis in this table repeats the analysis from Table 2-5. I regress the log of individual pitcher salary on the lag of earned run average, experience variables, and an indicator variable for whether the batter played on a team with a new stadium. The results in this table specifically show the coefficients on the new stadium indicator variable to show how this coefficient responds to a change in how a new stadium is defined (<5 years old, <7 years old, and < 10 years old).
3. t-statistics are in parentheses
   *=Significant at 10 percent level; **=Significant at 5 percent level; ***=Significant at 1 percent level

Curriculum Vitae

David B. Marzo was born on July 21, 1966, in Needham Massachusetts. He graduated from Spartanburg High School, Spartanburg, South Carolina, in 1984. In 1988, he received a Bachelor of Arts in mathematics from Georgetown University, Washington, District of Columbia. In 1997, he received a Master of Science in cost analysis from the Air Force Institute of Technology, Wright-Patterson Air Force Base, Ohio, where he graduated at the top of his class. He is an active duty officer in the United States Air Force, holding the rank of major. Upon completion of the doctoral program at George Mason University, he will be assigned to the Pentagon in Arlington, Virginia.